E Pluribus, Pauciores (Out of Many, Fewer): Diversity and Birth Rates

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Abstract: In the United States, local measures of racial and ethnic diversity are robustly associated with lower birth rates. A one standard deviation decrease in racial concentration (having people of many different races nearby) or increase in racial isolation (being from a numerically smaller race in that area) is associated with 0.064 and 0.044 fewer children, respectively, after controlling for many other drivers of birth rates. Racial isolation effects hold within an area and year, suggesting that they are not just proxies for omitted local characteristics. This pattern holds across racial groups, is present in different vintages of the US census data (including before the Civil War), and holds internationally. Diversity is associated with lower marriage rates and marrying later. These patterns are related to homophily (the tendency to marry people of the same race), as the effects are stronger in races that intermarry less and vary with sex differences in intermarriage. The rise in racial diversity in the US since 1970 explains 44% of the decline in birth rates during that period, and 89% of the drop since 2006.

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Since the middle of the 20th Century, the United States has experienced two major demographic changes. The first is a large increase in racial diversity. This is dramatic at the national level, but varies considerably at the local level. This trend arose both through large increases in immigration, and policies designed to reduce racial segregation. The second major change is a considerable decline in birth rates. Births per woman (total fertility rate, or TFR) have fallen by more than half, from approximately 3.6 total births per women in 1960, down to an all-time-low of 1.64 in 2020, which falls below the replacement birth rate needed to sustain the population. Declining fertility during a period of economic growth is prima facie surprising (Becker 1960). This is especially so for the recent declines since 2007, which are puzzling and hard to explain quantitatively (Kearney, Levin, Pardue 2022).

We pose a simple question which, to our understanding, has not been previously considered – are these two facts related? We argue that they are. We present evidence that birth rates are robustly lower in areas of greater local racial and ethnic diversity, after controlling for a wide array of potential confounding variables. We consider two slightly different aspects of racial variation within a community. The first, racial concentration, is a Herfindahl index of racial groups within a local area (as in Putnam 2007). Intuitively, this captures the difference between an area with many groups in small proportions versus mostly one major group, and maps to what is often just termed "diversity". The second measure is a consequence of racial diversity at the individual level, which we call "racial isolation" – the race share of the population for that person. Intuitively, this captures how many people in your area are "like you" in racial and ethnic terms.

There are strong reasons from the economics of marriage to predict that higher diversity may result in reduced birth rates. As the number of people of different races in each area has increased, people have fewer encounters with others of their own race. Various studies document that people on average have a preference for homophily – they prefer to marry those with similar characteristics, particularly people of the same race, even express such preferences in using reproductive technologies (e.g., Bedi 2000, Daniels and Heidt-Forsythe 2012, Hwang 2012, and many others). If the number of potential same-race partners drops in an area, then either one incurs higher search costs to find a good match, or the quality of matches

decreases, or both. While the evidence for homophily is large, the possibility that this may have implications that link the rise in diversity and the decline in birth rates does not seem to have been considered.

Using US Census and American Community Survey data since 1850, we find that both race Herfindahl and race share variables are robustly associated with higher birth rates. That is, being in a more racially concentrated area, and being part of a larger group within that area, are both associated with having more children, irrespective of the level of controls. With our full set of controls, a one standard deviation increase in race share predicts the average woman aged 18-40 has 0.064 more children, with t-statistics generally above 5 when clustered by state and year. For race Herfindahl, in our preferred specification a one standard deviation increase is associated with 0.044 more children. We construct these variables at the finest geographic level available– city, then county, then detailed metro area. As our baseline race definition, we use the census' broad racial classification plus a separate category for Hispanic/Latino ethnicity.

Our use of granular panel data combined with high dimensional fixed effects and demographic controls considerably narrows the set of plausible explanations for our findings. For instance, the use of state-by-year fixed effects helps mitigate concerns that the negative link between fertility and diversity is attributable to general economic or cultural attributes of a state. The use of race-by-state and race-by-time fixed effects precludes many explanations about general racial differences within a state. We control explicitly for demographics (education, income, citizenship, employment, marital status), demographics interacted with state and year fixed effects, local area attributes (population, college fraction, income, fraction recently moved to the area, employment, age), and local area attributes interacted with year fixed effects. The effect is large and highly significant in every specification. At a minimum, the most obvious omitted variables and their associated explanations do not seem to be driving the whole effect.

Because the Herfindahl measure is constructed as the sum of the squared fractions of each group, it is necessarily positively correlated with race share. The Herfindahl measure has the same value for everyone in an area that year, regardless of their race. However, even within a specific area and year, an individual's race share can vary further based on whether they belong to a more or less populous racial group relative to the area's overall racial composition. This means that for race share, local-area controls

can be replaced by an area-by-year fixed effect. If more diverse communities are bigger, richer, denser, have higher costs of raising children, or any other omitted factors, these are absorbed in this specification. Only variation *within* a local area and year is used, comparing larger and smaller groups within the area (after controlling for patterns in that race-by-state, race-by-year, etc.).

Racial isolation effects on birth rates survive these area-by-year fixed effects, and their inclusion does not greatly change the parameter estimates. The effects of racial isolation are not due to any general omitted characteristics of the area that apply to all residents, but appear to capture an effect directly related to the size of one's racial group. The consistent results across racial isolation and racial diversity measures suggest that they reflect a similar fundamental process, although directly testing this is challenging. While we do not explicitly argue for a causal interpretation of this relationship, we do not preclude one either. Diversity is both a cause and a consequence of various underlying factors, and even an exogenous change in diversity can have far-reaching effects on many aspects of an area that are hard to disentangle. To better understand the potential mechanisms and explanations behind the observed correlation, we employ targeted sub-tests designed to isolate specific factors and rule out alternative hypotheses, while acknowledging the inherent complexity of the relationship between diversity and birth rates.

Importantly, the negative association between diversity and birth rates is present throughout U.S. history. It holds before the 21st century, before the Civil Rights Act, before World War 2, before the 20th Century, and, most surprisingly, before the Civil War. That is to say, racial isolation significantly lowers birth rates even in periods when slavery was legal, in the 1850 and 1860 Censuses, with a one standard deviation increase in race share being associated with 0.33 more children. The parsimonious explanation is that whatever is driving the effect must be broadly present in many eras. These results militate strongly against explanations that focus on specific events in the history of race relations, whether this be the end of segregation, white flight, reconstruction, lynching, the "Black Lives Matter" movement, or anything else.

Second, the effects are present for many different racial groups. In our tightest specification, the effects of race share are positive and significant at the 10% level or better for seven out of ten groups (whites, blacks, native Americans, Chinese, Japanese, other, and two races – only other Asian/Pacific

Islander, Hispanic and three or more races are insignificant). In this specification, whites show the fourth smallest magnitude effect. Across specifications, the most uniformly positive and significant results are for whites, native Americans, and two races. In other words, the effect is not limited to whites, nor to a single racial group, nor is it easily attributable to simple narratives about black/white race relations. A likely explanation ought to apply to people of many different races.

Third, our findings are unlikely to be driven by selection effects related to mobility. For example, one possible explanation for our results is if younger people live in diverse areas but then move to racially homogenous areas when they have children. We redo the analysis for women who have not been geographically mobile - those living in the same state they were born in, or who have not moved in the years prior to the ACS survey. Across all subsamples, we observe consistent effects, suggesting that selective migration patterns are not the primary explanation for our results.

Fourth, we find that the result is present outside the United States, using international census data for countries that record racial classifications. Racial diversity is strongly associated with lower birth rates in Africa (South Africa, Mozambique, Zimbabwe), and also in a small sample of UK data. Central and South American countries show mixed evidence, with some having strong positive effects (Ecuador, El Salvador), others having significant negative effects (Uruguay, Cuba), and a number being insignificant (Jamaica, Brazil). These results do not reveal an obvious pattern of what drives the variation in effects across countries, but suggest that explanations unique to the U.S. are unlikely to be sufficient.

Next, we explore specific predictions of homophily. While homophily is a general pattern, it is unlikely that all races have the same revealed preference for same-race marriage at each point in time. If interracial marriage is more common for a given race and year, racial isolation should matter less for fertility. Second, within a race and year, interracial marriage rates also differ by sex, as women of a given race may "marry out" of their race at higher rates than men, or vice versa. This predicts different effects across the sexes – if women of a given race marry out more frequently, then racial isolation effects will bite more for men of that race than for women (as the men are more dependent on same-race women than those women are dependent on them). We find both predictions borne out in the data. More intermarriage reduces

the effect of racial isolation on fertility, and more intermarriage by women relative to men reduces the racial isolation effect for women of that race relative to men of that race. This is strongly consistent with homophily playing an important role in our effects and is not easily explainable by other channels.

To further test if our results are due to the difficulty of finding a desired partner, we examine other relationship outcomes. A one standard deviation increase in race share is associated with a 1.2 percentage point higher probability of a woman being currently married, a 1.2 percentage point higher probability of having ever being married, and a lower age of first marriage (by 2.3 months). It is somewhat negatively related to the probability of divorce, though the effects are weaker. Diversity effects do not appear to be limited to the narrow costs of raising children, but also to the difficulty of finding a martial partner.

Another prediction of homophily is that if people have preferences for similar partners along other demographic dimensions, we ought to find demographic share effects for other variables. The evidence here is more mixed – we find robust positive effects for income decile share that are around half to two thirds as large as the race share effect, consistent with the homophily among income levels documented in in Greenwood et al (2014). However, other variables like education and age do not show the same effects.

An alternative mechanism for our results is social trust. As Putnam (2007) describes: *"[I]n more diverse settings, Americans distrust not merely people who do not look like them, but even people who* do. *… Diversity seems to trigger not in-group/out-group division, but anomie or social isolation."*. Reduced social trust could contribute both to the difficulty of finding a partner, and choices over the number of children. While the predictions and metrics of trust are not as sharp as for homophily, we find that state level social trust measures are positively related to birth rates, and including them in the regressions reduces the race share effect by around a quarter to a third. This holds both when using Putnam (2007) measures of generalized trust from surveys, or when using more recent Facebook data, such as local volunteering rates.

Further evidence that homophily is unlikely to be the entire explanation comes from the fact that both race Herfindahl and race share measures show separate effects when included in the same regression. This holds even when the Herfindahl index is calculated only among races other than that of the individual in question. This helps us rule out the possibility that the Herfindahl index is merely capturing non-linear

effects of race share. Under homophily, the main question is the availability of potential same-race matches in one's vicinity, which is captured by the race share measure. It is unclear why, after conditioning on this, variation in the concentration of other races should matter, whereas under social trust, this aspect is important. That is, under homophily, if whites are 60% of the population in an area, this determines their chances of meeting and marrying each other, and it makes no difference whether the remaining 40% is a single race, or many races. Empirically, this variation matters (although it is subsumed by area fixed effects, and thus hard to tightly distinguish from other area traits). The importance of racial concentration does not point to social trust specifically, but it is consistent with it, and is difficult to explain with homophily alone.

Our final tests link time-series evidence of declining fertility rates within the U.S. to changes in diversity, related to the two motivating facts with which we began. The level of identification for time series changes is much weaker, but because the time series patterns are so stark, and the pattern so poorly explained in quantitative terms, it is an interesting question whether our cross-sectional evidence has enough bite to potentially be a driver of overall birth rates. We find that the average racial isolation explains 44% of variation in the US total fertility rate since 1971, and 89% since 2006. The predicted decline between 2006 and 2021 based on the coefficients is 0.426 children per woman, very close to the actual decline of 0.444. Diversity is large enough as a factor to potentially explain a large amount of birth rate time series variation, especially the most puzzling changes in recent years.

We argue that our evidence implies that diversity and birthrates have some fundamental tension between them. While homophily (and to a lesser extent, social trust) appear to be contributors to this relationship, it is less clear that they are the sole driving factors. Even if the pattern merely reveals other underlying factors that are not tied to race differences directly, these patterns are important to understand, as diversity and birth rates are some of the most important demographic changes of our age.

2. Literature Review

Our paper is related to literature on the economics of fertility. This starts with the seminal work of Becker (1960) which explained the decline in fertility during industrialization in the 19th century partly by

the declining value of children for agricultural work.[1] Becker and Lewis (1973) propose a quantity-quality tradeoff theory between having more children and investing more resources (e.g. education) into each one.

Recent surveys by Doepke and Tertilt (2016), Greenwood et al. (2017), and Doepke et al. (2022) describe the determinants of fertility. Women's decisions to have children are related to their labor market opportunities (Adsera 2005), and thus affected by drivers such as taxation (Guner et al. 2012, Bick and Fuchs-Schündeln 2017, and Borella et al. 2021), and access to education (Black, Devereux and Salvanes 2008, McCrary and Royer 2011). Fertility rates are also related to government spending on early childhood education programs (Olivetti and Petrongolo 2017), which function as a form of childcare, and such access is especially important for the decision to have multiple children (D'Albis et al. 2017).

Other research considers the role of family planning. Goldin and Katz (2002) argue that improved access to birth control for single women in the 1970s increased women's incentive to invest in a career and delay marriage and childbearing. Kearney and Levine (2009) find similar fertility effects from Medicaid subsidies of contraception, and Myers (2017) finds fertility effects from abortion access. Finally, other papers have examined costs of family formation, including child car seat laws (Nickerson and Solomon 2024), and mortgage deregulation (Hacamo 2021). Fertility is also affected by cultural influences like social attitudes to mothers working, (Kleven et al. 2019) and TV shows (Kearney and Levine 2015)

Relative to this literature, our paper makes several contributions. First, existing theories have considerable difficulty explaining the large and consistent decline in fertility in the US since 2007. As Kearney, Levine and Pardue (2022) describe it: "*The Great Recession contributed to the decline in the early part of this period, but we are unable to identify any other economic, policy, or social factor that has changed since 2007 that is responsible for much of the decline beyond that.*" We answer this challenge, and provide an explanation that is both new to the existing literature, and can potentially explain in quantitative terms the recent declines. Diversity differs conceptually from most of the existing birth rate

[1] This coincided with increasing economic growth, thus the prima facie puzzling inverse relationship between income and fertility. In recent decades, high-income countries no longer exhibit a negative correlation between income and fertility (Hazan and Zoabi 2015 and Bar et al. 2018).

drivers by being a property of a *local area,* whereas many of the studied aspects are narrow costs or benefits more closely related to child-raising and its substitutes that are targeted at the individual level.

Second, our findings provide an alternative explanation for the empirical demographic pattern whereby immigrants from high-fertility countries tend to converge over time to lower native levels (Dubuc 2012, Parrado and Morgan 2008, Sobotka 2008, Mulder and Wagner 2001, White, Moreno, and Guo 1995, Adsera and Ferrer 2015). The existing literature has mostly emphasized aspects of cultural transmission, but our results suggest an alternative mechanism – immigrants are typically moving from places where they have a high race share, to places where they have a low race share, and thus are expected to have lower fertility in subsequent generations.

Our paper also contributes to the research on homophily and matching in partner traits. As well as the previous cited literature on same-race preferences, there is also evidence of assortative matching based on income (Chiappori, Salanie and Weiss 2017, Greenwood et al. 2014, Fernandez et al. 2005, Schwartz and Mare 2005, and Chiappori et al. 2022). Our paper shows an important implication of such matching - when there is more local diversity along that dimension, marriage rates and birth rates are lower. While most of our evidence is about racial diversity, we find evidence for income-based diversity effects as well.

Our paper is also related to the literature in political economy on the relationship between ethnic diversity and social trust. This documents negative connections between ethnic diversity and favorable outcomes, such as civic engagement (Costa and Kahn 2003), the provision of public goods (Alesina et al., 1999), and self-reported trust levels (Putnam 2007). Dinesen et al. (2020), in their comprehensive meta-analysis, document a significant negative correlation between ethnic diversity and social trust across 1,001 estimates derived from 87 studies. They surmise the consistent negative correlation observed across various types of social trust aligns with Putnam's (2007) theory of anomie (social isolation), which posits a universal decline in trust across diverse social settings. Perhaps surprisingly, none of these studies discussed in the meta-analysis have examined fertility as an outcome. We contribute to these studies by documenting a new social outcome of diversity, and show a link to both trust and homophily as potential drivers.

2. Data and Variable Construction

2.1 Data Sources

Census data is obtained from IPUMS, a service of the Minnesota Population Center, which aggregates and standardizes census data from both US and international sources. U.S. census data is taken from decennial censuses from 1850 to 2010, plus yearly vintages of the American Community Survey (ACS) from 2000 through 2021. 1960 is excluded due to lacking local geographic information. International Census data is taken from IPUMS International, for nearly all samples where race data is non-missing (The United Kingdom, Mozambique, South Africa, Zimbabwe, Costa Rica, Cuba, El Salvador, Jamaica, Brazil, Colombia, Ecuador, Uruguay – we exclude very small samples from Suriname and Saint Lucia). U.S. Total Fertility Rate data and economic indicators (unemployment, GDP growth, inflation) are taken from the FRED website of the Federal Reserve Bank of St. Louis.

2.2 Main Variables

The main results of the paper relate local levels of racial diversity and racial isolation to birth rates. To do so, we have to unpack each of the component pieces – “birth rates”, “local”, “racial” and “diversity”.

For “birth rates”, we use this term to refer to the number of children a woman has living in her house at the time of the survey. We are not specifically measuring a rate, but the numbers will (in most specifications) control flexibly for age, among many other variables. While it would be possible to turn these numbers and ages of children into annual birth rates (as in Nickerson and Solomon (2024)), for most of the sample the race measures are only available at the same time and in the same survey as the birth counts, meaning there is not much ability to match diversity levels to birth choices at the time of conception.

By “local”, we conceptually refer to the community where the person resides, acknowledging that there is no single universally correct or optimal measure to define this. With arbitrarily fine data, one could imagine that the effect of houses nearby is different from the whole street, the neighborhood, the town, the county, and the state. Ideally, all these levels could be evaluated. With public census data, things are complicated on two dimensions. First, the collection of different geographic levels varies over time. As the

Data Appendix describes, the 1850 census collects information on city and detailed metro area. County information first starts in 1950, and metro area information ends in 2011. Some samples measure both city and county, others measure only one or the other. Even when multiple levels are available simultaneously, they do not nest each other. That is, there are cases of multiple counties within a city, as well as multiple cities within a county. Because county is generally finer (i.e. taking respondents where both city and county data are non-missing, the average number of cities per county is 5.35, whereas the average number of counties per city is 1.10), we take as our baseline measure

-First city, if this is available

-If no city information is available, then county (if this is non-missing)

-If neither city nor county is available, then detailed metro area.

-If none of these are available, the observation is dropped in the main analysis.

For our state-level diversity variables, because these do not require finer geographical information, we include all residents of a state (even if they lack other geographic information).

Second, "racial". There are numerous different ways to classify race. When using U.S. data, we follow the Census race classifications. For broad race measures, they include nine categories – white, black/African American, American Indian or Alaska native, Chinese, Japanese, other Asian or Pacific Islander, other race, two major races, and three or more major races. In addition, they also ask about Hispanic or Latino ethnicity, which interacts with the above. So, one can be Hispanic white, Hispanic black, Hispanic other, or any other combination.[2]

The aim here is to map to how people construct their own identity. As our primary grouping, we put all Hispanic/Latino respondents in a single, separate category. In this respect, our shorthand use of "race", unless otherwise qualified, refers to these ten groupings (the nine Census broad race groups, plus a tenth for Hispanic/Latino). The understanding is that this combines aspects of both race and ethnicity, in

[2] In 2021, the most common race labels self-chosen by Hispanic/Latino respondents are "other" or "two major races", due the U.S. classifications lacking a racial category that corresponds to Amerindians from Central and South America (whereas census race definitions in other countries include "Indigenous" in Brazil, Colombia, Costa Rica, Ecuador, El Salvador and Uruguay, and mixed-race versions like "Mestizo" in Ecuador, El Salvador, and Uruguay).

terms of how people construct their sense of identity. Including Hispanics/Latinos as a single group implicitly assumes that their sense of what it means to be surrounded by people “like them” covers other Hispanic/Latino people (rather than, say Hispanic whites feeling that Hispanic other are a different group). Some categories are unsatisfactory for this purpose no matter how it is done – it seems unlikely that “three or more major race” respondents only feel a sense of similarity with other “three or more major race” people, who may have entirely different combinations of race. All these measures are imperfect, and we later explore a number of other definitions, but using them does not materially affect our results.

Finally, the last metric is diversity and racial isolation. Both are linked by the idea of being surrounded by people who differ from you. Our notion of diversity relates to there being many different groups who are each a small fraction of the population – that is, the overall level of racial concentration. Importantly, we do not mean “diversity” merely as a shorthand for “not white”. This alternative measure aligns more closely with the race control variables themselves. We later also employ race as an interaction term in our analysis. Our main version of diversity using a Herfindahl index of racial concentration –the sum of the squares of the fraction of the population made up by each race in that area and year.

For racial isolation, we focus on the concept of being a small fraction of the population. We measure this using the race share variable, which represents the proportion of the local population with the same race as the woman in question. This is mechanically related to race Herfindahl, as shares are always zero or positive, and so race share squared (the addition to a Herfindahl) goes up with race share. The principal difference is that a race Herfindahl applies to everyone in an area, and so does not have any variation within an area and year. In this sense, while the Herfindahl measure maps most closely to the ordinary definition of “diversity”, it is necessarily hard to disentangle from other attributes in that community and year that greater variation in races may be associated with.

Given these considerations, we opt to use race share variable as our primary measure of racial diversity. There is a maintained assumption, which is hard to test, that race share and race Herfindahl are capturing similar underlying concepts – that is, that being a small racial group within your area draws on the same underlying mechanism as living in an area with many other different races. The consistency in

the direction of the results obtained using both variables strengthens the overall findings, even if the specific underlying mechanisms captured by each measure may differ. In practice, the results of the paper work similarly under either measure. The main difference is that race share allows for the addition of an area by time fixed effect. That is, we can control for all possible drivers of the number of children in a given area and year, and focus only on the difference between being part of a large racial group versus a smaller one.

Finally, in our base specifications, we measure race share and Herfindahl for the population aged eighteen and over, so that the number of children is not mechanically linked to attributes of those children.

3. Results

3.1 Base Effects of Diversity on Birth Rates

We begin by relating diversity and racial isolation to birth rates. Our main specification is:

$$\textit{Number of Children}_{i,j,t} = \mathrm{a} + \mathrm{b}_1 * \textit{RaceShare}_{i,j,t} + \mathrm{b}_2 * \textit{Controls}_{i,j,t} + e_{i,j,t}$$

Observations are taken for a woman *i*, living in area *j*, in year *t*. The list of controls varies according to specification, and we introduce them as they are added.

Table 2 Panel A presents the baseline results of race share on number of children. Column 1 is a univariate regression with no controls. In this specification, *RaceShare* is positively associated with the number of children, with a coefficient of 0.159, and significant at the 1% level with a *t*-statistic of 2.84 (with standard errors clustered by state and year). This regression includes years dating back to 1850. In terms of the economic magnitude, a one standard deviation increase in *RaceShare* is associated with the woman having 0.052 more children, on average.

Column 2 adds controls for *Race* (that is, the nine Census racial groups plus a tenth for Hispanic/Latino). *RaceShare* is necessarily correlated with race itself, as being a more numerous racial group (such as whites) makes it more likely that you will live nearby more people of the same race. When this is controlled for, the effect becomes larger and much more significant – the coefficient is now 0.708, with a *t*-statistic of 6.89. Intuitively, once we control for the fact that whites have a high race share in general, and low birth rates in general, the effect of *RaceShare* increases greatly. That is, comparing two

women of the same race shows a large effect of *RaceShare* on their number of children. We report two effect sizes. The first is the effect of one unconditional standard deviation of *RaceShare* (from column 1) multiplied by the coefficient. This is 0.230 more children, in this case. The second calculates the effect of a conditional standard deviation in *RaceShare.* That is, we regress *RaceShare* on the same fixed effects in the regression (here, just *Race*), and compute the standard deviation of the residuals. One standard deviation of this is associated with 0.123 more children. The difference between these two measures is approximately whether you use all the variation in *RaceShare* (and assume it has the same effect as the aspects already controlled for), or whether you just use the part remaining after stripping out the controlled-for components.

Column 3 adds fixed effects for *State* and *Year*. The coefficient is reduced to 0.435, but the *t*-statistic is similar at 6.92. The unconditional and conditional effects of a one standard deviation increase in *RaceShare* are 0.141 and 0.067 more children, respectively. Column 4 adds fixed effect controls for various demographic variables, collectively referred to as *Demographics*. This includes *Race* as before, but also categories for the woman's age, marital status, nationwide deciles of income, employment status, education, and citizenship status. With the full set of demographic controls, the earliest year for observations is now 1980. As before, the coefficient is reduced, to 0.310, but the *t*-statistic is increased to 7.30. Unconditional and conditional standard deviation effects are now 0.101 and 0.049 more children, respectively.

Column 5 keeps the *Demographics* variables, but replaces the *State* and *Year* fixed effects with an interacted *State-Year* fixed effect. The coefficient is now 0.241, with a *t*-statistic of 8.58. Column 6 replaces the baseline *Demographics* variables with interactions of *Demographics*State* and *Demographics*Year* (in addition to the *State-Year* effects). The coefficient increases to 0.291 with a *t*-statistic of 7.03.

Column 7 adds two new sets of controls related to local area metrics. First, we add dummy variables for the area type (county, city or metro area). Second, we calculate other metrics averaged at the local area: the fraction employed, the fraction college educated, average age, average income decile, and a z-score for

the fraction of people who have moved in the last one or five years, depending on data availability.[3] We collectively refer to these as *Area Traits*. Unlike other controls, these are calculated as linear effects. Adding these variables reduces the coefficient to 0.204, with a *t*-statistic of 6.58. The effect of an unconditional and conditional standard deviation of *RaceShare* is now 0.066 and 0.026 children, respectively.

Column 8 replaces the variables for area type with dummies that split each area type into population buckets (that is, creating *Area Type * Population Group*), where the grouping is either halves, quintiles or deciles, depending on the number of observations.[4] When comparing population characteristics across different geographic units, it is important to ensure that the units are comparable. The population of a city may not reflect the same meaningful "size" as the population of the surrounding metropolitan area. Instead, we operate under the assumption that populations of cities can be compared to other cities within the same year, counties with counties, etc., as they are comparable units. The coefficient is now 0.160, with a *t*-statistic of 5.00. Unconditional and conditional standard deviation changes in *RaceShare* result in 0.052 and 0.019 more children. Column 9 allows all area controls to be time-varying, so we replace *Area Traits* with *Area Traits * Year* and *Area Type * Population Group * Year*. Column 10 adds an *Area* fixed effect (e.g. for Cook County, Illinois). In both cases, the coefficients and significance are very similar to before.

Finally, we absorb all variation at the level of the area and year in Column 11, adding in *Area * Year* fixed effects. These replace all of the other area level controls (*Area Traits *Year*, and *Area Type * Population * Year*), as well as the *State*Year* fixed effects. In this specification, the only controls are *Demographics * (State, Year)* and *Area * Year*, with everything else being absorbed. The coefficient is largely unchanged from before, being 0.197 with a *t*-statistic of 5.88. An unconditional and conditional standard deviation increase in *RaceShare* results in 0.064 and 0.020 more children, respectively.

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[3] Different census years list either whether the respondent has moved in the last year, or the last five years. We first compute the average of each of these at the local area level. To make these comparable across years, we convert each into a z-score across all local areas that year. If both are available, we average the two.

[4] Each category (county, city, metro area) is split into percentiles based on the number of respondents that year from the area in question. If there are 20 or fewer area type observations in that year (e.g. fewer than 20 cities in the data that year), areas are split into high and low populations. If there are between 21 and 100 area type observations, they are split into quintiles. If there are more than 100, they are split into deciles.

With *Area*Year* fixed effects, we control for many general properties of an area and year that might influence birth rates. All the variation comes from differences in *RaceShare* between different groups in an area (i.e., comparing racial groups that are more numerous in that area versus less numerous). Because we also have *Race*Year* and *Race*State* (as part of the *Demographics* (State, Year)),* we are also comparing each group with the overall birthrate of that racial group in that state, and in that year. For example, if we consider Detroit, MI in 2007 (which is predominantly black), blacks in the city will have more children relative to blacks in Michigan generally, or blacks in 2007 generally, and whites in Detroit will have fewer children. Meanwhile, in Ann Arbor, which is predominantly white, the pattern will be reversed – whites will have more children than elsewhere in the state and year, and blacks will have fewer children. Other individual-level differences in the populations are controlled for in *Demographics * (State, Year*).

Because of this, our results cannot be attributed to any area-level traits that might be associated with diversity in general. That is, if more diverse areas are richer or poorer, have more jobs or fewer, are denser or sparser, or anything else – all of this is controlled for by the *Area *Year* effects. Racial isolation is now separate from the general level of diversity of an area, which is also absorbed. It is notable that the final step of *Area*Year* fixed effects changes the results very little. Controlling parametrically for the other aspects of the area produces very similar results to flexibly controlling for it with fixed effects.

Because the *RaceShare* variable includes both the "lots of different races in an area, each being small" aspect, and the "you personally are from a smaller group" aspect, we next turn to a version that captures only the first aspect. In Panel B, we replace the person's race share with a Herfindahl index of the different races in that area and year. This approach aligns more closely with the common understanding of diversity, but it comes with the limitation that the resulting measure only varies at the area-by-year level.

Column 1 shows that without controls, *RaceHerfindahl* positively predicts birth rates, with a coefficient of 0.590 and a *t*-statistic of 5.33. The unconditional effect of a one standard deviation increase in *RaceHerfindahl* (i.e., an area becoming more racially concentrated) is associated with 0.125 more children. The effect increases in magnitude and significance when adding race controls in column 2. Column 3 adds *State*Year* fixed effects, and the coefficient is 0.664, similar to the univariate specification,

now with a *t*-statistic of 15.02. Adding *Demographics*(State, Year)* in column 4 reduces the effect to a coefficient of 0.342, and a *t*-statistic of 14.95. Unconditional and conditional standard deviation changes in *RaceHerfindahl* are associated with 0.073 and 0.044 more children, respectively. Adding *Area Type* and *Area Traits* reduces the effect somewhat to 0.207 in column 5. Adding *Area Traits * Year* and *Area Type * Population * Year* in column 6 gives an effect of 0.121, with a *t*-statistic of 3.91, and effect sizes of 0.026 and 0.011 for unconditional and conditional standard deviation increases in *RaceHerfindahl.*

Next, we add *Area* fixed effects. Relative to Panel A, it is less clear what the right level of controls is. In the limit, adding in *Area*Year* will absorb all the variation, so this is not possible. Nonetheless, even when absorbing the average level of *RaceHerfindahl* via an *Area* fixed effect*,* we still find a positive (albeit smaller) and significant effect of 0.037, with a *t*-statistic of 4.16. The unconditional and conditional effects of a one standard deviation change in *RaceHerfindahl* are now 0.008 and 0.001 more children, respectively.

In Panel C, we include both *RaceShare* and *RaceHerfindahl* in the same regression. The specifications are the same as those in Panel B. In general, both variables show positive effects that are not subsumed by the other. The only exceptions are in column 1 (with no controls), where *RaceShare* loads negatively when race controls are absent, and in columns 6 and 7, where the addition of *Area* fixed effects and the *RaceShare* variable means that *RaceHerfindahl* is either zero or negative. In general, coefficients are somewhat reduced relative to the specifications with only one or the other variable, which makes intuitive sense given that the two variables have decent overlap, both conceptually and empirically.

One potential concern with Panel C is that *RaceHerfindahl* could be picking up non-linear effects of *RaceShare,* rather than a separate effect of concentration*.* Because *RaceHerfindahl* is made up of the sum of squared race shares, if *RaceShare* has additional effects beyond the linear specification we use, this may lead to *RaceHerfindahl* having measured effects even if racial concentrations do not matter directly. To rule out this possibility, in Panel D we replace the *RaceHerfindahl* with a different version, *OtherRaceHerfindahl*, which is the Herfindahl index just computed across all *other* races than the respondent's. In other words, this alternative version is orthogonal to whatever the respondent's own *RaceShare* is, and just represents the concentration of the remaining population. The results are generally

similar to those in Panel C, but somewhat stronger – the negative effect of *RaceShare* in column 1 disappears (and it is now positive and significant), while the zero and negative coefficients in columns 6 and 7 become positive and zero, respectively.

While *RaceHerfindahl* appears to be an important separate driver of birth rates, it is harder to distinguish from other area-level effects. This is seen in column 7, where including an area fixed effect causes both *RaceHerfindahl* and *OtherRaceHerfindahl* to lose their significance in Panels C and D. In other words, when both the area average level of *RaceHerfindahl* (or *OtherRaceHerfindahl)* is controlled for by an area fixed effect, *and* the level of *RaceShare* is controlled for, the remaining variation in racial concentration does not drive fertility. Recall that *both* sets of controls are necessary, however – in Panel B, *RaceHerfindahl* on its own still has significant effects with an area fixed effect included (but with *RaceShare* absent). For this reason, we argue that the bulk of the evidence supports the conclusion that racial concentration matters, over and above the level of the respondent's own racial share in the population. Nonetheless, a reader who is skeptical of what racial concentration is measuring absent the inclusion of an area fixed effect may not be convinced of a separate role for racial concentration. For this reason, in the remainder of the paper, we mostly focus on the *RaceShare* variable, due to the ability to add *Area*Year* fixed effects and get a tighter interpretation of what the variable measures. The results of the paper are generally similar if *RaceHerfindahl* is used instead, absent the inclusion of area fixed effects.

3.2 Alternative Specifications

Next, we explore a number of variations on the main specifications above. Table 3 Panel A constructs versions of the *RaceShare* variable using alternative definitions of race. These are i) omitting the Hispanic/Latino category, ii) using detailed race (instead of broad race) and omitting Hispanic/Latino, iii) using broad race and including Hispanic/Latino as an interaction rather than a separate category, iv) using detailed race and including Hispanic/Latino as an interaction, v) using ancestry share, instead of any race classification, and vi) using the base definition over the whole population, including those under eighteen.

For each variable, we include *Area*Year* and *Demographics*(State, Year)* controls (corresponding to Table 2 Panel A column 11). The effects are positive and significant at the 1% level in all cases.

Panel B constructs the *RaceShare* variable at different geographical levels. As geographic information varies across census years, limiting the analysis to only one type of geography alters the range of years included. To ensure that the controls remain comparable, in columns 1-4 we include *State*Year* and *Demographics*(State, Year)* (as other controls require area level information, which is not available for all specifications). Recall that in the base case, levels are constructed sequentially based on availability, so that (for instance) county is only used if city information is missing. For our first three measures, we use i) city, ii) county, and iii) detailed metro area, for all observations in each respective category. Next, iv) we reverse the priority order of city and county, so using county first, then city, and finally detailed metro area. In columns 1-4, all the relationships are positive and significant. We also v) use state level measures in columns 5 and 6, thus including all observations from the state, even those with missing information on any finer geography. For these two specifications, we omit *State*Year* controls, as these would map to within-area versions of the variable (and thus not be comparable to the earlier columns). State level measures are only significant at the 10% level, however, in column 5. This suggests that it is more local geography that drives these effects. Consistent with this notion, in column 6 we add both the baseline *RaceShare* variable and the state level version in the same regression. The baseline local version is highly significant while the state level metric exhibits a somewhat negative effect.

Panel C explores different levels of weighting. The baseline regressions weight every response equally, which necessarily draws more observations both from larger population areas, and from recent years. In column 1, we weight every *Area*Year* combination equally, regardless of the number of respondents. In column 2, we weight every year equally. In column 3, we weight each year equally, but also weight each observation in that year according to the census household weights. In columns 4-6, we apply census household weights within the area when constructing the *RaceShare* variable, and apply the same observation-level weighting choices as before. The results are positive and highly significant in all specifications, which include *Demographics*(State,Year)* and *Area*Year*.

3.3 Mobility and Selection

We now turn to tests designed to shed light on what the baseline result of the paper is measuring. One class of explanation is selection effects based on mobility. When people have children, or are thinking about having children, they may desire to be in areas with more people of their own race, even if that does not directly affect how many children they have. This could come from a direct preference for being around people of the same race (a social form of homophily), or being drawn to particular amenities in an area that are more favored by one race over another. If these are complements to having children, then people might relocate because of the child choice, rather than the child choice being affected by the diversity.

To test this, in Table 4 we re-run our tests using various metrics of women who are less likely to have moved. If a woman has not moved at all, then it is not a concern than she moved based on diversity and fertility decisions. Information on mobility-related questions is collected unevenly over years, so we measure mobility in different ways, limiting the sample to women who are less likely to be mobile. All regressions include controls for *Demographics*(State, Year),* and *Area*Year.*

When limiting the sample to women who are less mobile, we still find positive and statistically significant effects at the 1% level in all specifications. Column 1 limits the sample to women living in their state of birth. Recall that the coefficient on *RaceShare* in the analogous specification (Table 2 Panel A Column 11) is 0.197. For women living in their state of birth, the coefficient is slightly lower, at 0.161. Column 2 limits the sample to women who haven't moved in the past year. The effect is similar to Table 2, at 0.200. Column 3 limits the sample to women who haven't moved in the past five years, and finds a somewhat lower coefficient of 0.124. Column 4 takes women who either haven't moved in the past year, or haven't moved in the past five years (with surveys generally asking either one question or the other, but not both). The effect is 0.191. Finally, if any of the three measures of being less mobile is grounds for inclusion, the effect is 0.190, with a *t*-statistic of 5.42. The robustness of the effects across all subsamples suggests that mobility and selection are unlikely to be the primary drivers of the relationship between race

share and birth rates. While these factors may contribute to the overall effect, as evidenced by the slightly lower coefficients in some specifications, they do not appear to be the dominant explanation for the findings.

3.4 Time Periods

Next, we consider the effect across different time periods. While this is not a direct test of a specific mechanism, the very long time period of our data allows us to implicitly test the importance of a variety of different theories. For instance, one might imagine that the effect is concentrated in the Obama presidency, or the Civil Rights Act, or policies during Reconstruction. In Table 5, we evaluate the baseline effect in different periods of the U.S. census dating back to 1850. The set of controls here is limited by the available in the early periods – to ensure comparability, in all years we use the controls available in 1850. In Panel A, this is *State*Year,* and *(Age, Race) * (State, Year)*. In Panel B, we also include an *Area*Year* fixed effect.

The periods studied include 1850-1860 (column 1), 1870-1890 (column 2), 1900-1940 (column 3), 1950-1970 (column 4), 1980-1990 (column 5), and 2000-2021 (column 6). In Panel A, we find large and significant results in all specifications. In terms of magnitudes, the coefficients in Panel A are generally decreasing across over time, ranging from 1.889 in 1850-1860, to 0.515 in 2000-2021. However, due to the smaller number of observations in the early period, the significance is lower, with 1850-1860 having a *t*-statistic of 2.01, significant at the 10% level (with all other periods significant at the 1% level). If magnitudes are measured in terms of marginal effects of a one standard deviation change in race share, the largest effect is in 1850/60 with 0.33 children, decreasing to a marginal effect of 0.158 in 2000-2021. The higher variation in *RaceShare* in later years offsets some of the decrease in coefficients, so the difference in marginal effects is not as large.

Panel B includes *Area*Year* fixed effects. Now the first several columns are no longer statistically significant, with significance being stronger starting in 1950. Interestingly, the coefficients now somewhat increase over time, although they are stable from 1950 onwards. In this respect, it is not clear what to infer about the magnitude of the effect over time, as the answer depends on what level of controls is applied.

Despite being not directly tied to a particular theory, Table 5 is in fact greatly constraining of what explanations can be operating. If one assumes that the same pattern in the data is driven by the same cause, then when variation between areas is included, that cause must be operating before the Civil War, during Reconstruction, during the Gilded Age, during both World Wars, during the Civil Rights Era, at the end of the Cold War, and throughout the 21st century. Even if one only believes specifications using just within-area variation, the cause must be present since 1950. Theories that emphasize contemporary aspects of race relations, regardless of the specific aspect they focus on, will generally face challenges in explaining the pervasive presence of this effect throughout U.S. history. The consistency of the observed relationship between racial diversity and birth rates across various historical periods suggests that the underlying mechanisms are likely to be more fundamental and deeply rooted than those captured by theories primarily concerned with current racial dynamics.

3.5 Effects Across Races

To further investigate potential mechanisms, we examine how the baseline effect varies across different racial groups. While the main results control for race (and its interactions with state and year) as a determinant of birth rates, here we focus on the interaction effects. Many theories about the impact of diversity primarily emphasize black/white race relations, and concepts like the historical legacy of slavery. An important test for such theories, if they are indeed the primary drivers of the observed effect, is what prediction they make for other racial groups. By examining the interaction effects across a wide range of racial groups, we can better assess the applicability and explanatory power of theories that predominantly focus on specific racial dynamics.

We consider these possibilities in Table 6. We run a similar set of specifications to Table 2 Panel B (though as we include race interactions, all specifications require *Race* fixed effects). When thinking about the effect across different races, there are two ways to consider the effect:

-Does it hold in the most stringent specification (i.e. with *Area*Year* fixed effects?)

-Does it hold across a wide range of different specifications?

To begin with the first aspect, namely the effect across races in the most stringent specification, Table 6 Column 7 shows the results for interactions of *RaceShare* with all ten racial groups, after adding controls for *Demographics*(State, Year)* and *Area*Year*. Given that these ten racial groups encompass all the possible categories for the baseline regressions, the ten interactions subsume the base effect, and thus the interpretation is whether *RaceShare* variable has a significant effect on birth rates specifically within that racial group. The results show that the effect is positive and significant at the 10% level for 7 out of 10 groups (with only Hispanic, other Asian / Pacific Islander, and three or more races not being significant). In terms of magnitude, whites show the fourth smallest effect, and two races and other have the largest.

However, when considering the consistency of the effect across different specifications for each racial group, a somewhat different picture emerges. For white respondents, the effects are positive and significant in every specification, which is relatively unsurprising, given the baseline result for all respondents is very strong, and whites constitute the largest racial group in the sample. In contrast, the consistency of the effect varies more notably across specifications for other racial groups, suggesting potential differences in the robustness of the relationship between racial diversity and fertility depending on the level of controls and specific racial population being examined. Results are generally also positive and significant for Native Americans/Indians, blacks, and two races. Effects are generally positive but not always significant for Hispanic other and three or more races, with the only significant values being positive. Other Asian/Pacific islander is insignificant in all specifications. Japanese and Chinese switch from positive and significant to negative and significant across specifications.

The interpretation of the most stringent specification is the clearest, namely that the effect is present in some degree for nearly all racial groups once the largest number of other alternative drivers of birth rates are accounted for. However, the interpretation of the other specifications is less clear, as it requires either taking a definitive stance on the precise (and smaller) number of controls that should be included or simply considering the generality of the results. Even in this case, it is not clear what explanation would show the most reliable results for whites, blacks, native Americans, and two races.

3.6 International Results

Another class of explanation is aspects that are unique to U.S. history or the U.S. context of race relations. An important test of such theories is whether the results are present in other countries. To test this, we use IPUMS international data, for nearly all countries that collect race information, excluding only Saint Lucia and Suriname where the small sample sizes make geographic measurements challenging. In Panel A, we consider African countries (Mozambique, South Africa, Zimbabwe) plus the UK (the only European country we observe). In Panel B, we consider Central American countries (Costa Rica, Cuba, El Salvador, Jamaica). In Panel C, we consider South American countries (Brazil, Colombia, Ecuador, Uruguay).

IPUMS codes up geography at the coarse level and the fine level, which roughly corresponds to states and sub-state units (cities, counties, etc.). We measure *RaceShare* at the finest level available, usually fine geography, but sometimes coarse geography.

For race definitions, we use the groupings collected by each country, with full details provided in the data appendix. When multiple samples are available for a given country, we add *Year* interactions where applicable. For each country, where possible we use three specifications:

i) *Coarse Geography* Year* and *Demographics.*

Here, coarse geography approximately corresponds to state. Our list of *Demographics* includes whatever is available out of: an urban dummy, race, marital status, age, educational attainment, and employment status.

ii) *Coarse Geography* Year, Demographics*(Coarse Geography, Year), Ln Population Density*Year*

We also include the natural log of population density, interacted with year.

Finally, in iii) we also include *Fine Geography *Year.*

Exact details of what is included for each country are in the data appendix.

Panel A examines the effects for the UK and African countries, and finds a generally positive relationship, except when using *Fine Geography *Year* effects. The UK is unusual, having only a single sample and only coarse geography measures (so *RaceShare* is at the coarse geography level). Standard error clustering is also at the coarse geography level, or at the fine geography level where this is available (since

the small number of time periods makes clustering by time either inadvisable or outright impossible). Column 1 includes *Demographics* and *Ln Population,* and finds a positive and significant effect. This disappears in column 2, when *Coarse Geography* controls are added (which, recall, are at the same level as the *RaceShare* variable, so are more equivalent to the fine geography controls in other countries). In columns 3, 6 and 9 we find positive and significant effects for *RaceShare* in Mozambique, South Africa, and Zimbabwe, when controlling for *Coarse Geography *Year* and *Demographics.* In columns 4, 7 and 10, these remain positive and significant, and actually increase in magnitude, when controls are added for *Demographics*(Coarse Geography, Year)* and *Ln Population Density*Year.* When controls for *Fine Geography *Year* are added (looking only at variation between races in the same area, like Table 2 Panel A Column 11), the effect is insignificant in Mozambique and Zimbabwe, but still significant in South Africa.

Panel B shows effects for Central America. Costa Rica and Cuba show mixed results, being zero in the first specification, negative and marginally significant when adding *Demographics*(Coarse Geography, Year)* and *Ln Population Density*Year*, but positive and significant when adding *Fine Geography * Year*. El Salvador shows a similar pattern to Panel A – positive and significant in the first and second specifications, but insignificant when *Fine Geography *Year* effects are added. Jamaica shows insignificant effects in all specifications.

Panel C shows the effects for South America. Brazil shows the only reliably negative effects across all specifications. These are small in magnitude compared with other countries, but the large number of observations (over 16 million) makes them significant. Colombia shows insignificant results in all specifications. Ecuador shows the Panel A pattern, of being positive and significant in the first two specifications, but insignificant when *Fine Geography *Year* controls are added. Uruguay is negative and significant in the first two specifications, but positive and significant in the third.

Overall, these results show that the effect in international data is not as ubiquitous as in the U.S., but neither is it limited only to U.S. data. The number of countries where the results are "some positive and significant, none negative and significant" is six, whereas "some negative and significant, none positive and significant" is only one. Moreover, the pattern of which countries show positive effects does not tell an

obvious story. While U.S. descriptions of race relations often focus on the interaction between black and white populations, the results are present in the UK (which has far fewer blacks), and African countries (which are overwhelmingly black), but not in Jamaica (which is also overwhelmingly black) nor Brazil (which has a large black population by U.S. definitions, but also a rather different conception of racial categories). We consider the question of understanding all these sources of variation to be interesting, but beyond the scope of the paper. Instead, these results serve as an indication that the results may arise from forces that occur in other counties as well, but not universally so.

3.7 Potential Explanations – Marriage and Divorce

Next, we explore the implications of racial diversity for other relationship outcomes, particularly those relating to marriage. One version of the baseline explanation is that the effect of diversity on the number of children is narrowly related to some aspect of the costs of raising children, and thus represents a decision only along this dimension. If instead the impact on the number of children is related to broader issues that affect relationships, then we might expect to see effects on rates of marriage and divorce. These additional relationship outcomes are useful tests of the underlying mechanisms, being consistent with both trust and homophily explanations. At a minimum, if the effects extend beyond childbearing decisions and influence marriage formation and dissolution, it suggests that the role of racial diversity in shaping family structures and dynamics is more nuanced than a narrow focus on the costs of childrearing might suggest.

We analyze this question in Table 8. In Panel A, we consider the probability that a woman is currently married at the time of survey. Using the same specifications as in Table 2 Panel B, we find that higher levels of *RaceShare* are associated with a greater likelihood of the woman being currently married, significant at the 1% level in all specifications. The effect sizes for an unconditional change in *RaceShare* on probability of being married range from 1.2 to 6.2 percentage points (in columns 7 and 1 respectively). In Panel B, we instead consider the effect on whether a woman was ever married (that is, the variable equals one if the woman is married, divorced or widowed, and zero if she is single). The effects here are generally

similar in magnitude and statistical significance to Panel A. A one standard deviation unconditional increase in *RaceShare* is associated with higher chances of ever being married by 1.2 to 6.2 percentage points.

Panel C examines the age at first marriage. This is for a subset of the data, as we limit the sample to women who are currently married, and who have only been married once (as others lack data on the age of first marriage). The effects here are the most reliable of the four panels. A one standard deviation unconditional increase in *RaceShare* is associated with getting married between 6.6 months earlier (column 1) and 2.3 months earlier (column 7).

Panel D examines the probability of being divorced, conditional on getting married. In other words, the dummy variable now equals one if the woman is divorced, and zero if she is married or widowed (with single now being omitted). While there are some effects of *RaceShare* on lower divorce rates in the early specifications, the effects are smaller and less consistent, with the versions with tighter controls showing no effect. A one standard deviation change in *RaceShare* results in divorce probability ranging from 1.9 percentage points lower (column 1) to 0.4 percentage points higher (column 2).

Overall, these results reinforce that diversity is negatively associated with marriage rates, with the main drivers being whether and when you get married, more so than whether you get divorced. This reinforces the conclusion that diversity is associated with broader relationship effects, rather than just child-rearing costs, narrowly defined.

3.8 Sex and Race Differences in Interracial Marriage

Next, we consider one of the channels that might contribute to a causal interpretation of the main result. In particular, we consider the role of homophily in relationship preferences. There is considerable evidence of a general tendency of people to prefer to marry someone similar to them. Similarity in terms of race is one of the strongest of these. If people have a preference for marrying someone of the same race, then the fraction of the population around them who are of that race is an important determinant of the chances of them finding a suitable marriage partner. As well as there being evidence of assortative matching along racial dimensions (e.g., Hwang 2012), the evidence from sperm donation suggests a preference for

same-race traits in donors (Daniels and Heidt-Forsythe 2012), even when the man will not be present in the woman's life (and thus correlated aspects like partner income cannot be driving the choice).

A number of results already presented are consistent with this possibility of homophily as a driver. First, results about diversity being associated with the chances of getting married, and how long it takes to get married. Second, the fact that the across-area coefficients are declining over time in Table 4 (although, it is noted, the specification with *Area*Year* does not show this pattern), consistent with greater social acceptance of interracial relationships and marriage.

It is tempting to attempt to address this problem by controlling for whether the woman married someone of a different race, but this is unlikely to be sufficient. If the pool of same-race partners shrinks, then one may end up instead marrying someone of the same race, but of a worse quality match than they might have otherwise gotten in a larger pool. The challenge lies in the fact that the characteristics determining the quality of a match are often difficult to observe and quantify. As a result, simply controlling for interracial marriage may not fully capture the impact of a reduced pool of same-race partners on the quality of marriages and on relationship outcomes such as fertility. To gain a more comprehensive understanding of the relationship between racial diversity and family formation, it is important to consider not only the direct effects on interracial marriage but also the more subtle ways in which partner availability and match quality may be affected by the size and composition of the pool of potential partners.

We turn to two additional tests of this hypothesis. The essential component of homophily as an explanation is the preference for same-race marriage over interracial marriage. But this preference for marrying within one's own race is unlikely to be uniform across all racial groups and historical periods. To take a stark example – the census category of "three or more races" is unlikely to be a source of strong homophily, whereby people of three or more races only want to marry someone else of three or more races (regardless of what those three races actually are), rather than someone of two races (classified as a different racial group), or someone of one of those three racial groups, or anyone else. More broadly, different racial groups likely have different norms about the importance of marrying someone of the same race. If one is

from a group where interracial marriage is strongly discouraged, then it should be of larger importance to live around more people of the same race, so as to have a larger dating and marriage pool.

One minor complication of this question is that the raw level of interracial marriage is somewhat mechanically related to *RaceShare* itself. For instance, in a state where whites make up 90% of the married population, it is simply not possible for them to have a large interracial marriage rate (whereas the 10% remaining population could, in principle, all marry someone of a different race). Instead, we compute the abnormal intermarriage rate, by comparing the actual intermarriage rate nationwide for that race and year, with the simulated distribution if all married people that year paired up randomly. We compute 1000 simulations of random pairings, and compute a z-score of the actual intermarriage rate, minus the simulation average, divided by the simulation standard deviation. We interact this variable with the *RaceShare* variable. We add the univariate variable for abnormal marriage only in the specification that does not include *Race*Year* fixed effects, as these subsume the abnormal intermarriage rate.

However, intermarriage rates may also be reflecting other differences across races that matter for other reasons. For this purpose, we turn to a second, sharper prediction, namely sex differences in intermarriage rates. In particular, men and women of the same race often "marry out" of their race at different rates. Each man that marries a woman of a different race reduces the pool of marriageable men for women of his own race. If the population sizes of the sexes are roughly equal, then neither sex will have a surplus of potential partners as a baseline. This highlights the essential aspect that intermarriage rates for men increase the pressure on women of the same race, and vice versa. Unlike the previous tests, any overall traits that are common to both men *and* women of that race, however arising, should not affect this rate.

We measure this by calculating the interracial marriage gender ratio for each race and year. This ratio is obtained by dividing the fraction of married women of a given race who have a husband of a different race by the corresponding fraction of married men of the same race who have a wife of a different race. For these tests in Table 9 on intermarriage rate, and sex differences in intermarriage rate, we take both men and women ages 18-40 (as opposed to the other tables, which only include women). We replace the controls for *State*Year, Demographics*(State, Year)* and *Area*Year* with interactions with *Sex,* so that these effects can

vary between men and women (so we have *Sex*State*Year, Sex*Demographics*(State, Year),* and *Sex*Area*Year* respectively). The only exception here is that we cannot include *Race*Sex*Year* because this would absorb the variation we are using, so instead we use *Race*Year* and *Race*State* as before.

These results are presented in Table 9. The first four columns show the effect of the abnormal intermarriage rate. We find that higher levels of abnormal intermarriage are associated with a lower effect of *RaceShare.* That is, *RaceShare*AbnormalIntermarriageRate* is negative and significant in three of the four specifications. The interpretation here is that when men and women of that race are more likely to marry people of different races, then it makes less difference to their average number of children whether they are living near people of the same race or not. The only exception to this is when we add controls for *Sex*Area*Year*, when the effect becomes smaller and insignificant.

Next, we consider the effect of sex differences. Our main variables of interest are thus *RaceShare*IntermarriageSexRatio* and *RaceShare*IntermarriageSexRatio*Male*. The former estimates the effect of women marrying out more on women of that race, while the latter is the increased effect of *RaceShare* for men relative to women as the female rate of marrying out increases.

In columns 5-8, we consider the effect of the *IntermarriageSexRatio* and its interaction with *Male*. We observe that *RaceShare*IntermarriageSexRatio*Male* is positive and significant in all specifications. That is, when women marry out at higher rates relative to men, then it matters more for men whether they are living in a high *RaceShare* area or not, relative to how much it matters for women. The key prediction is the triple interaction, *RaceShare*IntermarriageSexRatio*Male.* This is positive and significant in all specifications. Even regardless of the other effects of the sex ratio, the reduction in fertility is greater for males. The coefficient on *RaceShare*IntermarriageSexRatio* is negative, but not generally significant.

3.9 Trust

Next, we turn to the second major theory that could explain our results, namely social trust. Higher levels of racial diversity are associated not just with lower direct levels of trust (i.e. survey respondents' answers as to whether you can generally trust people or not), but also with a variety of other

aspects of social capital related to trust. As Putnam (2007) describes:

"In areas of greater diversity, our respondents demonstrate:

• Lower confidence in local government, local leaders and the local news media.

• Lower political efficacy – that is, confidence in their own influence.

• Lower frequency of registering to vote, but more interest and knowledge about politics and more participation in protest marches and social reform groups

• Less expectation that others will cooperate to solve dilemmas of collective action (e.g., voluntary conservation to ease a water or energy shortage).

• Less likelihood of working on a community project.

• Lower likelihood of giving to charity or volunteering.

• Fewer close friends and confidants.

• Less happiness and lower perceived quality of life.

• More time spent watching television and more agreement that 'television is my most important form of entertainment'."

It is plausible that some or all of these factors are related both to the likelihood of people finding a suitable marriage partner, and their choice of how many children they would like to bring into the world. The predictions for this hypothesis are not as sharp as those for homophily. However, the two aspects that are the most straightforward are that i) trust levels should be positively associated with birth rates, and ii) controlling for trust levels should reduce the effect of *RaceShare*.

We consider two ways of measuring trust. The first is the direct measure used in the 2006 Social Capital Benchmark Survey, where respondents are asked "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?". We take the state level average fraction responding "people can be trusted" as a proportion of those answering either this response, or "you can't be too careful" (with responses of "it depends", "don't know" or refusal to answer being omitted). Because we have the state level information only for 2006, we apply these survey responses to all years for that state. As a result, in these regressions, we cannot include state fixed effects, or state

interactions with other fixed effects. Instead, our controls are *Demographics*Year*. In this respect, the coefficients have much less controlled for than above. Nonetheless, our interest is how much these state metrics explain fertility, and how much they reduce the effect of *RaceShare* under this level of controls.

We present these results in Panel A. Because our trust metrics are state level, we compare them to both state level versions of *RaceShare* (in columns 1-6), and with the baseline local version (in columns 7-12). Finally, because our trust measure is only from a single year, we also vary the time sample to be years increasingly matched to the timing of the trust measure – all years (columns 1 and 2), 2001-2010 (columns 3 and 4), and 2006 only (columns 5 and 6). We find the first prediction confirmed – in all specifications, states with higher levels of general trust (*StateLevelTrust)* in 2006 have higher birth rates, when controlling for *Demographics*Year*. Second, we find that controlling for *StateLevelTrust* also reduces the effect of *RaceShare(State)* and *RaceShare(Base).* Columns 1, 3 and 5 compute the coefficient on *RaceShare(State)* for the years in question and observations where we can match *StateLevelTrust* data. Columns 2, 4 and 6 show the same coefficient once *StateLevelTrust* is controlled for. In column 2, the effect of *RaceShare(State)* is reduced by 21% once we add trust controls. In the 2001-2010 sample, the reduction is slightly larger, at 24%, in column 4. In 2006 alone, the reduction is slightly larger still, at 26%. Columns 7 to 12 show similar effects, albeit smaller reductions, when the baseline geography definitions are used. This is consistent with broad state-level trust measures having less ability to drive out geographically tighter measures of diversity.

In Panel B, we examine a different set of social capital metrics. These are taken from Chetty et al. 2022, which uses Facebook data to construct county-level measures of various social capital metrics. We focus on the three major measures of that paper – the volunteering rate (i.e. the fraction of people who participating in a volunteer organization), friendship clustering (the chances that, if A and B are friends, and C is friends with B, that A is also friends with C), and economic connectedness (the share of above median income friends by people with below median income). As these measures are at the county level, we compare them with county-level versions of *RaceShare*. We compare the baseline *RaceShare* variable in the same periods and counties that we have social capital measures, and then add the three social capital measures. We do this for all years (columns 1-2), 2011-2021 (columns 3-4) and 2021 only (columns 5-6).

Comparing the univariate *RaceShare* effect with the version with all three social capital measures included, the coefficient is reduced by 39%, from 0.318 to 0.200 in the full sample (with similar effects in other year ranges). Volunteering rates have directionally positive effects in all specifications, but are only significant when only using 2021 data. Friendship clustering and economic connectedness are negative but insignificant in sign. Overall, these results are consistent with social trust being a contributing factor to the racial diversity / fertility effect, with estimates ranging from 20-37% of the effect coming from this channel.

3.10 Diversity Along Other Dimensions

Next, we examine whether population shares have similar effects across demographic variables in general. This represents a robustness test for the possibility that *RaceShare* is just proxying for that person's similarity to their local area along other, correlated dimensions. Second, it examines a related aspect of homophily– do people have higher birth rates if they are surrounded by people who are similar along other dimensions, or just primarily race? Conceptually, homophily does not require that the preference for similarity be equally strong on, or even present on, every dimension of possible similarity. Nonetheless, examining more variables helps distinguish between a version of homophily where race is just one example among many, or a version where race is one of the primary aspects of marital preferences.

We construct analogous *[Variable]Share* variables for different demographic aspects of similarity. In Table 11 Panel A, we consider education, income decile, and age. Because shares depend on how coarsely or finely the groups are defined, coefficients are not directly comparable. For age, we calculate the share as the fraction of the population that is between two years younger and ten years older than the woman. We consider the effect of these variables under different fixed effects combinations as before.

Panel A shows that *EducationShare* has a sign that is positive, but loses significance with additional controls. *IncomeDecileShare,* in columns 5-8, shows a positive and statistically significant effect in all specifications, including with *Area*Year* fixed effects. Being of similar income to the people around you has the greatest similarity with race in its effect on fertility outcomes. The unconditional effect of a one

standard deviation increase in *IncomeDecileShare* ranges from 0.024 more children in column 5 to 0.028 more children in column 6. In columns 9-12, *AgeShare* shows inconsistent effects across specifications.

In Panel B, we consider two other dimensions of similarity, namely the fraction of people that share the same status of being born in the US or not, and the fraction of people that share the same citizenship status. Both *USBornShare* and *CitizenShare* shows positive and significant effects in the first two columns, but insignificant negative effects once more controls are added. Overall, these effects show a similarly positive and consistent homophily effect for income, but weaker or inconsistent effects for the other variables. This suggests that race is not unique as a variable where homogeneity is associated with higher fertility, but neither do all important demographic variables have the same effect.

In Panel C, we consider these variables together. We consider the same four specifications, showing coefficients in columns 1-4, and the effect of a one standard deviation unconditional change in each of the variables in columns 5-8. Importantly, *RaceShare* is positive, significant, and of a similar magnitude in all specifications. Adding in other area-level controls has a larger effect on the *RaceShare* coefficients relative to Table 2 Panel A in the early columns, because it represents a relatively greater addition of new control variables. However, with the *Area*Year* fixed effects in column 4, the *RaceShare* coefficient is 0.191, very close to the 0.197 in the equivalent specification in Table 2 Panel A column 11.

The effect of *RaceShare* is about two and a half times as large as *IncomeDecileShare*, with effect sizes between 0.061 and 0.073 additional children, compared with 0.022 to 0.023 additional children for *IncomeDecileShare*. Finally, a number of the variables that show weak or inconsistent "univariate" effects (i.e., as the only *[Variable]Share* variable) show very different patterns after controlling for other aspects of similarity. *CitizenShare* is now large and economically significant, but *USBornShare* is now negative and significant, as is *EducationShare.* This suggests that if our main result is driven by homophily, then a number of variables show somewhat complicated effects, whereby a trait that is desirable at a univariate level may be undesirable once other correlated aspects of matches are controlled for.

The fact that income share is the next most reliable measure is consistent with the considerable evidence for assortative matching based on income (Chiappori, Salanie and Weiss 2017, Greenwood et al.

2014, Fernandez et al. 2005, Schwartz and Mare 2005, and Chiappori et al. 2022). It is less obvious that this represents an explicit preference for homophily (i.e., it is not clear that lower income people explicitly prefer their partners to also have low income). Our result can also arise in matching models like Becker (1971), where everyone wants a richer partner, but has to be richer themselves to attract them. Even in this model (which lacks homophily), higher diversity may still lower marriage and birth rates, if the rich view their local low-income partner possibilities as being worse than the outside option of remaining single.

3.11 Time Series Evidence

Finally, we consider directly the original motivating question we began with – the time series changes in overall fertility and diversity. This aspect is somewhat implicit in the Table 2 specification with no controls, but we wish to test the overall question using simpler methods – how much of the overall change in fertility could plausibly be related to the increase in diversity and racial isolation?[5] In Table 11 Panel A, our dependent variable is the total fertility rate for the US, from FRED, since 1961. We relate this to the average of the *RaceShare* variable in the year before (when most conception decisions would have been made). We include as controls various economic variables lagged by a year: inflation, GDP growth, and unemployment. In column 1, the univariate effect of average *RaceShare* is 1.440, with a *t*-statistic of 3.86. In terms of economic magnitude, there are two ways to think of this. First, the R-squared of the regression is 0.439, indicating that a substantial amount of year-to-year variation is explained. Secondly, we consider the full time-series change over the period (a drop in TFR of 0.602), versus the predicted change in TFR based on the changes in average *RaceShare* and the regression coefficient, and get a predicted change of 0.393. That is, the variable explains 65.3% of the overall drop in fertility.

Column 2 adds economic controls. The effect increases in both magnitude and significance, and now explains 117% of the overall decline. Because the level of geographic measurement varies over the sample, columns 3, 4 and 5 show the effect of average *RaceShare* measured for cities only, counties only,

[5] In the time series tests, it is not possible to distinguish between the effects of national averages of *RaceShare* and *RaceHerfindahl*, as the time series correlation is 0.99. In other words, at the national level, higher average diversity and higher average racial isolation are extremely strongly linked, and thus their effects cannot be separated.

and states. The effect is insignificant for cities only, but significant for the remainder. Predicted changes are 245.8%, 85.2% and 117.0% of actual declines respectively. Columns 6-10 limit the sample to 2006 onwards, where the continuous availability of annual data allows us to use Newey-West standard errors, here with a lag of five years. The effects are now larger in coefficient and significance during this period. The univariate R-squared in column 6 is now 88.6%. Predicted changes relative to actual amount to between 94.8% and 115.3% of the actual changes.

In Panel B, our dependent variable is the unadjusted average number of children for respondents. This is sensitive to other factors like the age profile of the women being sampled, but it has the advantage of being easy to construct back to 1850. In columns 1-4, we find that all geographic measures work as univariate predictors since 1850 with *t*-statistics above 6. R-squared values range from 0.571 to 0.796, and predicted changes as a fraction of actual are 85.8%, 70.4%, and 83.6% for base, city and state respectively (with county being an odd outlier at explaining 3892%, partly due to the smaller number of observations).

Finally, in columns 5-8, we repeat the analysis using only decennial observations to mitigate the potential influence of the numerous annual observations available since 2000. The results obtained from this restricted sample are consistent with our previous findings.

It goes without saying that with aggregate time series changes it is hard to say for sure what their drivers are, and the ability to make causal statements is very limited. Nonetheless, to the extent that one believes in a potential causal channel from the tighter cross-sectional tests already discussed, these tests serve to show that the magnitudes of the time series changes are considerable, and that changes in diversity may be important variables for helping quantitatively explain the decline in birth rates that we observe.

4. Conclusion

In this paper, we document a new and important stylized fact linking the central demographic changes of our time. Women living in areas of higher racial diversity robustly have fewer children. We do not explicitly argue that this represents a causal relationship, but the obvious non-causal explanations have considerable difficulty explaining the range of facts we document. The effect is present in every period that

U.S. Census data is easily obtainable, so it is not an artefact of modern race relations. It is present for many different races of women, so is not just related to black/white race relations. It holds (unevenly) in other countries, so while it is not an inevitable human universal, it is also not limited to the U.S. Diversity is not only associated with the direct costs of raising children, but also other relationship outcomes like the likelihood of getting married, and the age at which marriage occurs. The findings suggest that the impact of racial diversity extends beyond the narrow scope of childrearing expenses and influences multiple aspects of family formation and stability.

What alternatives are left that fit all the facts above? The strongest of these is preferences for homophily in partner choice, and we present evidence specifically consistent with this, from differences in interracial marriage across races, and between the sexes within a race. These additional results are hard to explain under competing theories. More speculative, but potentially also important, is the role of social trust. Putnam (2007) links this to racial concentration, the more direct measure of diversity. Our results also show a negative relationship between racial concentration and birth rates, and this generally holds controlling for race share. The relationship between racial isolation specifically, and what these other aspects of racial concentration are capturing, is an important avenue for future studies.

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Table 1 – Summary Statistics

This table presents summary statistics for the main variables used in the paper. Data is taken from U.S. Census and American Community Survey files from 1850 to 2021, obtained from IPUMS, for all women aged 18-40 at the time of survey. *Number of Children* is the number of children the woman has living at home at the time of the survey. *Race Share* is the fraction of the local area population of the same race as the women, where race is the nine broad racial groups classified by the census, plus a tenth for Hispanic/Latino. *Race Herfindahl* is the sum of squared percentages for each racial group in that local area. Panel B presents breakdowns by year. Local areas are defined as being first city (if available), then county (if available), then detailed metro area. The number of local areas and total respondents is shown, along with race shares for the ten groups in that year. A blank value means that classification was not collected at the time.

Panel A - Whole Sample								
	N	Mean	Std Dev	Min	25th Pct	50th Pct	75th Pct	Max
Number of Children	7,156,888	0.98	1.30	0	0	0	2	9
Age	7,156,888	28.93	6.68	18	23	29	35	40
Race Share	7,156,888	0.557	0.324	0.000	0.231	0.635	0.855	1.000
Race Herfindahl	7,156,888	0.587	0.212	0.177	0.411	0.572	0.763	1.000

Panel B - By Year														
Year	N	# Areas	Race Share Mn	Race Share SD	White	Black	Amer. Indian	Chinese	Japanese	Asia Pac.	Other	Two Races	Three Races	Hispanic
1850	7,418	72	0.917	0.191	0.9520	0.0453								0.0027
1860	12,548	105	0.938	0.168	0.9656	0.0308	0.0002	0.0002						0.0033
1870	18,509	176	0.891	0.212	0.9240	0.0715	0.0001	0.0002						0.0042
1880	26,751	259	0.885	0.217	0.9187	0.0750	0.0000	0.0009						0.0054
1900	60,426	513	0.894	0.209	0.9243	0.0701	0.0001	0.0002	0.0000					0.0053
1910	85,664	693	0.886	0.216	0.9182	0.0739	0.0001	0.0004	0.0008	0.0002				0.0063
1920	98,633	398	0.871	0.227	0.9141	0.0740	0.0002	0.0003	0.0017	0.0005				0.0092
1930	142,954	1,111	0.842	0.246	0.8919	0.0889	0.0004	0.0005	0.0019	0.0005				0.0160
1940	140,756	234	0.834	0.251	0.8904	0.0908	0.0003	0.0005	0.0010	0.0002				0.0168
1950	192,521	259	0.777	0.274	0.8494	0.1169	0.0004	0.0010	0.0017	0.0003	0.0001			0.0301
1970	403,112	187	0.716	0.301	0.7961	0.1290	0.0022	0.0037	0.0052	0.0048	0.0013			0.0578
1980	320,561	511	0.687	0.309	0.7591	0.1334	0.0047	0.0054	0.0040	0.0137	0.0011			0.0784
1990	332,815	565	0.647	0.316	0.7182	0.1234	0.0058	0.0099	0.0045	0.0257	0.0009			0.1118
2000	241,894	159	0.486	0.297	0.5609	0.1504	0.0049	0.0155	0.0042	0.0458	0.0022	0.0192	0.0014	0.1954
2003	20,136	61	0.572	0.323	0.6345	0.0777	0.0067	0.0089	0.0114	0.0502	0.0026	0.0162	0.0037	0.1880
2005	312,914	651	0.555	0.323	0.6213	0.1167	0.0051	0.0149	0.0040	0.0483	0.0031	0.0125	0.0011	0.1731
2006	322,868	650	0.542	0.319	0.6083	0.1210	0.0052	0.0158	0.0036	0.0496	0.0030	0.0134	0.0012	0.1789
2007	324,831	650	0.537	0.318	0.6037	0.1193	0.0049	0.0158	0.0037	0.0517	0.0030	0.0146	0.0012	0.1822
2008	322,150	650	0.529	0.317	0.5959	0.1205	0.0050	0.0159	0.0033	0.0529	0.0028	0.0159	0.0016	0.1863
2009	325,889	650	0.522	0.315	0.5887	0.1220	0.0048	0.0163	0.0033	0.0540	0.0027	0.0170	0.0015	0.1898
2010	328,152	650	0.503	0.301	0.5746	0.1254	0.0051	0.0169	0.0030	0.0555	0.0023	0.0188	0.0018	0.1966
2011	327,101	650	0.500	0.307	0.5620	0.1329	0.0060	0.0181	0.0030	0.0546	0.0022	0.0201	0.0023	0.1989
2012	274,379	504	0.473	0.300	0.5329	0.1304	0.0050	0.0206	0.0033	0.0614	0.0024	0.0208	0.0031	0.2200
2013	280,088	504	0.474	0.300	0.5390	0.1259	0.0049	0.0209	0.0034	0.0618	0.0026	0.0216	0.0035	0.2165
2014	278,666	504	0.468	0.299	0.5336	0.1253	0.0046	0.0223	0.0030	0.0637	0.0026	0.0226	0.0036	0.2187
2015	281,817	504	0.466	0.298	0.5326	0.1222	0.0046	0.0230	0.0029	0.0637	0.0025	0.0230	0.0034	0.2220
2016	282,583	504	0.464	0.297	0.5326	0.1183	0.0043	0.0240	0.0028	0.0647	0.0028	0.0238	0.0038	0.2229
2017	288,755	504	0.461	0.297	0.5321	0.1142	0.0044	0.0244	0.0028	0.0674	0.0029	0.0248	0.0038	0.2231
2018	290,629	504	0.462	0.297	0.5348	0.1111	0.0045	0.0245	0.0028	0.0668	0.0030	0.0252	0.0039	0.2235
2019	287,380	504	0.463	0.299	0.5388	0.1069	0.0044	0.0257	0.0026	0.0680	0.0029	0.0262	0.0037	0.2208
2020	234,678	504	0.440	0.292	0.5181	0.1059	0.0042	0.0271	0.0025	0.0703	0.0047	0.0405	0.0052	0.2215
2021	289,310	504	0.428	0.286	0.5008	0.1039	0.0041	0.0263	0.0025	0.0734	0.0058	0.0438	0.0052	0.2344

Table 2 – Number of Children and Racial Diversity

This Table presents the baseline relationship between the number of children a woman has and various measures of local levels of racial diversity. Data is taken from the U.S. decennial census from 1850 to 2000, and from the American Community Survey from 2001 to 2021. Observations are taken for women ages 18-40 at the time of survey who have non-missing geographic information for either city, county, or detailed metro area. The dependent variable is the number of children the woman has. In Panel A, the main independent variable is *Race Share*, the fraction of the population in the local area who are of the same race/ethnicity as the woman ("race", as a shorthand). Race is constructed as ten categories, with nine categories for the broad racial groups (if the respondent is not Hispanic or Latino) and a tenth category for Hispanic/Latino. Local area is measured first as county (if present), then city (if county is missing), then metro area (if both city and county are missing). Fixed effects are included as labeled for race, state, year, state by year, demographics (age, marital status, education, race, employment status, income decile, and citizenship), demographics by state and year, area type (i.e., county, city or metro), deciles of population within that area type, deciles of population by year, area (where area is measured at the same level as the race share), and area by year. Area parametric controls are included as average income decile, average age, fraction employed, and a *z*-score for the fraction of residents who moved in the last 1 or 5 years (depending on data availability). These are also interacted with year fixed effects. The earliest year for data availability (given the set of controls) is noted. The effect on the number of children of a one standard deviation change in race share is indicated, both for an unconditional one standard deviation change across all observations, and a conditional standard deviation – a one standard deviation change in the residual after first regressing race share on the set of fixed effects in the regression. In Panel B, the race share variable is replaced with a race Herfindahl index for that area and year. In Panel C, both the race share and race Herfindahl index are included. In Panel D, the Herfindahl Index is computed only among races other than the respondent's own race (so it measures concentration among the races other than your own). Standard errors are double clustered by year and state. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Panel A - Race Share										
	Dependent variable is number of children at time of survey										
Race Share	0.159***	0.708***	0.435***	0.310***	0.241***	0.291***	0.204***	0.160***	0.166***	0.194***	0.197***
	(2.84)	(6.89)	(6.92)	(7.30)	(8.58)	(7.03)	(6.58)	(5.00)	(5.26)	(5.89)	(5.88)
Effect of 1 σ change (unconditional)	0.052	0.230	0.141	0.101	0.078	0.094	0.066	0.052	0.054	0.063	0.064
Effect of 1 σ change (conditional)	0.052	0.123	0.067	0.049	0.038	0.037	0.026	0.019	0.020	0.020	0.020
Race	N	Y	Y	N	N	N	N	N	N	N	N
State	N	N	Y	Y	N	N	N	N	N	N	N
Year	N	N	Y	Y	N	N	N	N	N	N	N
State-Year FE	N	N	N	N	Y	Y	Y	Y	Y	Y	N
Demographics FE	N	N	N	Y	Y	N	N	N	N	N	N
Demographics*(State, Year) FE	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Area Type	N	N	N	N	N	N	Y	N	N	N	N
Area Traits	N	N	N	N	N	N	Y	Y	N	N	N
Area Traits * Year FE	N	N	N	N	N	N	N	N	Y	Y	N
Area Type * Population FE	N	N	N	N	N	N	N	Y	N	N	N
Area Type * Population FE *Year	N	N	N	N	N	N	N	N	Y	Y	N
Area FE	N	N	N	N	N	N	N	N	N	Y	N
Area * Year FE	N	N	N	N	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,596	5,967,585	5,967,585	5,967,585	5,967,585	5,967,585	5,967,585
R-squared	0.002	0.017	0.041	0.313	0.392	0.400	0.402	0.402	0.402	0.404	0.405

Panel B - Race Herfindahl							
	Dependent variable is number of children at time of survey						
Race Herfindahl	0.590***	0.819***	0.664***	0.342***	0.207***	0.121***	0.037***
	(5.33)	(7.76)	(15.02)	(14.95)	(8.76)	(3.91)	(4.16)
Effect of 1 σ change (unconditional)	0.125	0.174	0.141	0.073	0.044	0.026	0.008
Effect of 1 σ change (conditional)	0.125	0.156	0.083	0.044	0.023	0.011	0.001
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.009	0.022	0.042	0.393	0.402	0.402	0.404

Panel C - Race Share and Race Herfindahl							
Race Share	-0.162***	0.262***	0.236***	0.143***	0.157***	0.159***	0.197***
	(-5.02)	(4.65)	(4.81)	(4.68)	(4.24)	(4.54)	(5.85)
Race Herfindahl	0.751***	0.674***	0.530***	0.261***	0.112***	0.023	-0.080***
	(6.05)	(6.15)	(11.57)	(9.13)	(3.56)	(0.62)	(-3.48)
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.010	0.023	0.043	0.393	0.402	0.402	0.404

Panel D - Race Share and Race Herfindahl Across Other Races							
Race Share	0.244***	0.694***	0.528***	0.319***	0.241***	0.182***	0.203***
	(4.42)	(7.17)	(6.88)	(7.57)	(7.17)	(5.61)	(5.37)
Other Race Herfindahl	0.691***	0.569***	0.310***	0.235***	0.094***	0.036*	0.014
	(6.31)	(5.28)	(3.48)	(4.81)	(3.83)	(2.05)	(0.66)
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,114,973	7,114,973	7,114,972	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.014	0.024	0.042	0.393	0.402	0.402	0.404

Table 3 – Alternative Constructions of Diversity Variable

This Table presents alternative versions of the main regressions in Table 2. Panel A considers alternative definitions of race. This includes i) omitting the Hispanic/Latino category ii) detailed race measures but omitting the Hispanic/Latino category iii) race interacted with Hispanic/Latino, iv) detailed race measures interacted with Hispanic/Latino v) using ancestry instead of race and ethnicity, and vi) using the baseline measure (broad race plus a Hispanic category) only for the population aged 18 and older). Panel B varies the geographic region race share is measured at, including i) city only, ii) county only, iii) metro area only, iv) State, and v) a combined measure in the different order, namely city first, then county, then metro area. Panel C explores different weighting schemes, including i) weighting each area by year equally, ii) weighting each year equally, iii) using census household weights, and iv) using census household weights when constructing the race share variable. Standard errors are double clustered by year and state. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Different Race Definitions						
Race Share (No Hispanic Category)	0.183***					
	(7.91)					
Race Share (Detailed Race, No Hispanic Category)		0.188***				
		(8.37)				
Race Share (Hispanic Interaction)			0.209***			
			(6.15)			
Race Share (Detailed Race, Hispanic Interaction)				0.223***		
				(6.89)		
Ancestry Share					0.312***	
					(6.02)	
Race Share (Only 18+ population)						0.197***
						(5.88)
Demographics*(State, Year)	Y	Y	Y	Y	Y	Y
Area * Year	Y	Y	Y	Y	Y	Y
Observations	5,967,586	5,966,368	5,967,568	5,962,421	5,161,861	5,967,585
R-squared	0.403	0.405	0.405	0.409	0.413	0.405

Panel B - Different Region Measures						
Race Share (City)	0.205***					
	(5.99)					
Race Share (County)		0.302***				
		(7.66)				
Race Share (Metro Area)			0.175***			
			(3.28)			
Race Share (City, then County, then Metro)				0.286***		
				(6.54)		
Race Share (State)					0.151*	-0.236**
					(1.86)	(-2.42)
Race Share (Baseline - County, then City, then Metro)						0.215***
						(6.43)
State*Year	Y	Y	Y	Y	N	N
Demographics*(State, Year)	Y	Y	Y	Y	Y	Y
Observations	1,531,597	5,008,007	3,127,919	5,967,585	9,267,466	1,531,597
R-squared	0.393	0.401	0.394	0.400	0.392	0.392

Panel C - Weighting						
Race Share (Baseline - Unweighted)	0.210***	0.197***	0.190***			
	(6.47)	(5.87)	(6.22)			
Race Share (HH Weighted)				0.208***	0.199***	0.193***
				(6.88)	(6.17)	(6.55)
Sample Weighting	Area*Year (Indiv)	Year (Indiv)	Year (HH Weight)	Area*Year (Indiv)	Year (Indiv)	Year (HH Weight)
Demographics*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area * Year FE	Y	Y	Y	Y	Y	Y
Observations	5,967,585	5,967,585	5,959,683	5,967,585	5,967,585	5,959,683
R-squared	0.402	0.406	0.393	0.402	0.406	0.393

Table 4 – Mobility

This Table examines whether the main effects are due to selection effects based on mobility. We conduct similar versions of the main regressions in Table 2, but limit the sample to various categories of women less likely to have relocated: i) those living in the state they were born in, ii) those who haven't moved in the past year, iii) those who haven't moved in the past 5 years, and combinations of these. Standard errors are double clustered by year and state. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share	0.161***	0.200***	0.124***	0.191***	0.190***
	(5.05)	(5.13)	(3.87)	(5.11)	(5.42)
Selection	Living in State of Birth	No Move in Last Year	No Move in Last Five Years	No Move in Last One or Five Years	Any of Previous
Demographics*(State, Year) FE	Y	Y	Y	Y	Y
Area * Year FE	Y	Y	Y	Y	Y
Clustering	State, Year	State, Year	State	State, Year	State, Year
Observations	3,211,983	3,861,439	423,389	4,284,878	5,100,548
R-squared	0.405	0.406	0.457	0.412	0.407

Table 5 – Different Time Periods

This Table examines how racial diversity is associated with number of children in different time periods of US history. Specifications from Table 2 are run separately for i) 1850-1860, ii) 1870-1890, iii) 1900-1940, 1950-1970, 1980-1990, and 2000-2021. Panel A includes state by year fixed effects as well as age and race both interacted with state and year. Panel B also includes area by year fixed effects. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - No Area*Year Fixed Effects						
Race Share	1.889*	1.015***	0.949***	1.054***	0.552***	0.515***
	(2.01)	(2.83)	(3.19)	(3.91)	(6.37)	(5.92)
Period	1850-1860	1870-1890	1900-1940	1950-1970	1980-1990	2000-2021
Effect of 1 σ change (unconditional)	0.335	0.215	0.227	0.310	0.173	0.158
State*Year	Y	Y	Y	Y	Y	Y
(Age, Race)*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area*Year FE	N	N	N	N	N	N
Observations	19,929	105,637	467,974	595,615	653,356	5,314,218
R-squared	0.302	0.252	0.181	0.282	0.261	0.269

Panel B - With Area*Year Fixed Effects						
Race Share	-1.162	0.049	0.069	0.355*	0.338***	0.321***
	(-1.23)	(0.32)	(0.62)	(1.93)	(5.35)	(5.60)
Period	1850-1860	1870-1890	1900-1940	1950-1970	1980-1990	2000-2021
Effect of 1 σ change (unconditional)	-0.206	0.010	0.017	0.104	0.106	0.098
State*Year	Y	Y	Y	Y	Y	Y
(Age, Race)*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area*Year FE	Y	Y	Y	Y	Y	Y
Observations	19,929	105,635	467,969	595,615	653,356	5,314,218
R-squared	0.311	0.267	0.198	0.289	0.273	0.283

Table 6 – Effects by Race

This Table examines how the effect of racial diversity on number of children varies with the race of the woman. Observations are taken for women ages 18-40, in US census surveys from 1850 to 2021. Specifications from Table 2 are run with interactions between the baseline race share variable, and then ten racial ethnic groups we consider (from census categories): white, black, native American, Chinese, Japanese, Asian/Pacific Islander, other, two races, three or more races, and Hispanic. Controls are the same as in Table 2. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share * White	1.000***	0.703***	0.327***	0.414***	0.263***	0.264***	0.185***
	(7.38)	(7.46)	(8.62)	(9.74)	(6.18)	(5.90)	(3.70)
Race Share * Black	0.084	0.111	0.136**	0.070**	0.131***	0.137***	0.272***
	(0.76)	(1.26)	(2.22)	(2.50)	(3.33)	(3.75)	(4.24)
Race Share * Native American	0.474***	0.436***	0.705***	0.474***	0.167	0.188*	0.252*
	(3.63)	(4.81)	(9.99)	(4.56)	(1.67)	(1.79)	(1.90)
Race Share * Chinese	-0.506**	-0.015	-0.067	-0.077	0.673***	0.704***	0.957***
	(-2.18)	(-0.07)	(-0.63)	(-0.39)	(4.09)	(4.24)	(7.46)
Race Share * Japanese	0.584**	-0.855***	0.156*	-0.025	0.630	0.681	1.114**
	(2.53)	(-5.18)	(2.06)	(-0.06)	(1.35)	(1.47)	(2.26)
Race Share * Asian / Pacific Islander	-0.259	-0.166	0.053	-0.110	0.171	0.182	0.141
	(-0.90)	(-1.12)	(0.35)	(-0.83)	(1.36)	(1.42)	(0.83)
Race Share * Other	0.710	15.308***	4.475	3.121	3.540	3.700	5.838*
	(0.20)	(3.05)	(1.39)	(0.95)	(1.05)	(1.13)	(2.07)
Race Share * Two Races	0.554	0.854	0.257	1.692**	2.723***	2.879***	2.011***
	(0.47)	(1.60)	(0.60)	(2.19)	(4.20)	(4.33)	(4.34)
Race Share * Three or More Races	2.535***	1.122*	1.381***	-0.435	-0.648	-0.626	-1.003
	(5.44)	(1.75)	(4.40)	(-0.50)	(-0.67)	(-0.64)	(-1.11)
Race Share * Hispanic	0.122	0.093	0.137*	0.137*	-0.013	-0.000	0.158
	(1.38)	(1.13)	(2.03)	(1.73)	(-0.24)	(-0.00)	(1.56)
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Area FE	N	N	N	N	N	N	N
Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585	5,967,585
R-squared	0.020	0.042	0.392	0.400	0.402	0.403	0.405

Table 7 – International Results

This Table examines the relationship between local racial diversity and the number of children a woman has, for different countries around the globe. All countries with census data on IPUMS that contain information on race are included, Observations are taken for all women age 18-40 at the time of the survey. The dependent variable is the number of children the woman has. The independent variable is the fraction of the local area population of the same race as the woman. Geography is measured at the finest level available (usually "level 2" on IPUMS, generally corresponding to regions within a state, but sometimes "level 1", generally corresponding to a state, if there is no level 2 information). Race is measured according to whatever definition is used in the country in question. Controls are included for level 1 by year (colloquially, "state-year"), demographics, demographics by state and year, log population density by year, and local region (i.e., level 2) by year. Demographics variables include whichever is available for that country, out of urban status, marital status, race, employment status, age, and educational attainment. Full country-level information on race definitions and controls is included in the Data Appendix. Panel A examines the United Kingdom and three countries from Africa – Mozambique, South Africa, and Zimbabwe. Panel B examines countries from Central America – Costa Rica, Cuba, El Salvador and Jamaica. Panel C examines countries from South America – Brazil, Colombia, Ecuador and Uruguay. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - UK and Africa

Country	UK		Mozambique			South Africa			Zimbabwe		
Race Share	0.528***	0.141	0.501***	0.664***	-0.006	0.158***	0.323**	0.319***	3.360***	3.887***	-0.560
	(5.05)	(0.87)	(3.40)	(6.46)	(-0.07)	(3.28)	(2.81)	(7.49)	(2.67)	(3.62)	(-0.09)
State-Year FE	N	Y	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	Y	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	N	N	Y	Y	N	Y	Y	N	N	N
Demographics*State FE	N	N	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	Y	N	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	N	N	Y	N	N	Y	N	N	Y
Number of Years	1	1	2	2	2	4	4	4	1	1	1
Race Share Level	State	State	Local	Local	Local	Local	Local	Local	Local	Local	Local
Clustering	State	State	Local	Local	Local	Local	Local	Local	Local	Local	Local
Observations	92,397	92,397	638,099	638,096	638,096	1,797,315	1,797,315	1,797,315	122,944	122,938	122,938
R-squared	0.439	0.440	0.286	0.299	0.305	0.293	0.301	0.302	0.340	0.349	0.353

Panel B - Central America												
Country	Costa Rica	Costa Rica	Costa Rica	Cuba	Cuba	Cuba	El Salvador	El Salvador	El Salvador	Jamaica	Jamaica	Jamaica
Race Share	-0.158	-0.212*	0.270*	0.012	-0.076*	0.064**	0.308***	0.319***	0.026	0.241	0.241	-0.045
	(-1.26)	(-1.77)	(2.00)	(0.80)	(-1.85)	(2.23)	(3.50)	(3.72)	(0.36)	(0.42)	(0.42)	(-0.08)
State-Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	N	N	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	Y	Y	N	Y	Y	N	N	N	N	Y	Y
Demographics*State FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Number of Years	2	2	2	2	2	2	1	1	1	3	3	3
Race Share Level	Local	Local	Local	Local	Local	Local	Local	Local	Local	State	State	State
Clustering	Local	Local	Local	Local	Local	Local	Local	Local	Local	State	State	State
Observations	148,777	148,777	148,777	383,755	383,755	383,755	108,368	108,364	108,364	39,723	39,723	39,723
R-squared	0.432	0.444	0.446	0.268	0.273	0.275	0.405	0.414	0.417	0.301	0.301	0.303

Panel C - South America												
Country	Brazil	Brazil	Brazil	Colombia	Colombia	Colombia	Ecuador	Ecuador	Ecuador	Uruguay	Uruguay	Uruguay
Race Share	-0.038***	-0.056***	-0.015**	0.017	-0.027	0.012	0.200**	0.108*	0.016	-1.311***	-1.869***	1.056***
	(-3.70)	(-4.33)	(-2.05)	(0.28)	(-0.77)	(0.40)	(2.45)	(1.81)	(0.39)	(-5.08)	(-8.35)	(2.81)
State-Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	N	N	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	Y	Y	N	N	N	N	Y	Y	N	Y	N
Demographics*State FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Number of Years	4	4	4	1	1	1	2	2	2	1	1	1
Race Share Level	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local
Clustering	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local
Observations	16,032,064	16,032,064	16,032,064	678,567	678,566	678,566	492,708	492,706	492,706	52,118	52,103	52,103
R-squared	0.440	0.469	0.473	0.385	0.395	0.399	0.394	0.402	0.403	0.386	0.399	0.402

Table 8 – Racial Diversity, Marriage and Divorce

This Table examines how local levels of racial diversity affect outcomes related to marriage and divorce. Observations are taken for women ages 18-40, in US census surveys from 1850 to 2021. The main independent variable is race share – the fraction of the local population that is the same racial/ethnic group as the woman. Controls are the same as those in Table 2. In Panel A, the dependent variable is a dummy equal to one if the woman is currently married, and zero otherwise. In Panel B, the dependent variable is a dummy equal to one if the woman ever married (that is, if she is either currently married, widowed, or divorced), and zero otherwise. In Panel C, the sample is limited to women who got married, and the dependent variable is a dummy equal to one if there are currently divorced. In Panel D, the same is limited to women who are currently married, and on their first marriage. The dependent variable is the age at which they got married. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Currently Married							
Race Share	0.192***	0.046**	0.081***	0.112***	0.040***	0.042***	0.036***
	(4.57)	(2.25)	(4.02)	(4.39)	(3.44)	(3.67)	(2.95)
Effect of 1 σ change (unconditional)	0.062	0.015	0.026	0.036	0.013	0.014	0.012
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,118,413	7,118,412	5,967,596	5,967,587	5,967,587	5,967,587	5,967,587
R-squared	0.030	0.067	0.298	0.310	0.314	0.315	0.319

Panel B - Ever Married							
Race Share	0.191***	0.050**	0.074***	0.119***	0.041***	0.043***	0.036***
	(5.11)	(2.57)	(3.76)	(4.54)	(4.14)	(4.54)	(3.36)
Effect of 1 σ change (unconditional)	0.062	0.016	0.024	0.039	0.013	0.014	0.012
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,118,413	7,118,412	5,967,596	5,967,587	5,967,587	5,967,587	5,967,587
R-squared	0.023	0.068	0.376	0.387	0.392	0.392	0.396

Panel C - Age at First Marriage (Given Currently Married, Married Only Once)							
Race Share	-1.790***	-1.382***	-1.124***	-1.443***	-0.686***	-0.691***	-0.627***
	(-5.96)	(-7.61)	(-8.87)	(-6.98)	(-5.24)	(-5.34)	(-5.48)
Effect of 1 σ change (unconditional) in Months	-6.6	-5.1	-4.1	-5.3	-2.5	-2.5	-2.3
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	1,730,380	1,730,380	1,730,380	1,730,368	1,730,368	1,730,368	1,730,368
R-squared	0.026	0.054	0.234	0.243	0.247	0.248	0.254

Panel D - Divorced, Given Married							
Race Share	-0.060**	0.012	-0.032***	-0.019**	0.003	0.002	0.007
	(-2.39)	(0.98)	(-3.99)	(-2.64)	(0.48)	(0.34)	(1.00)
Effect of 1 σ change (unconditional)	-0.019	0.004	-0.010	-0.006	0.001	0.001	0.002
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	3,883,186	3,883,186	3,061,482	3,061,468	3,061,468	3,061,468	3,061,468
R-squared	0.019	0.038	0.127	0.136	0.137	0.138	0.141

Table 9 – Effects of Interracial Marriage by Race and Sex

This examines whether the effects of racial diversity on the number of children are impacted by measures of interracial marriage. In this table, we consider both men and women, ages 18-40, using the same survey data from 1850 to 2021, and take as the dependent variable the number of children assigned to that person. In columns 1-3, we interact the race share measure with a measure of abnormal levels of interracial marriage for that racial group and year. This is done by taking the set of all men and women aged 18-50 in that survey year, and computing the number from that race who are currently married to someone of a different race (using our previous definitions of race). Next, we randomize the races of all men and women in that sample who are currently married, and compute the number of interracial marriages we have under this random pairing. We compute 1000 such simulations, and use these to create a mean random rate of interracial marriage, and a standard deviation. The anormal interracial marriage measure is the actual rate minus the randomized mean, divided by the randomized standard deviation. This is interacted with race share, and included separately in column 1 (whereas in all other columns, the base variable is absorbed by the race-by-year fixed effect). In columns 5-8, we consider sex differences in the interracial marriage rate. For each race and year, we compute the number of women from that race who are married to someone of a different race, divided by the number of men from that race who are married to someone of a different race. This intermarriage sex ratio is then interacted with race share, and race share interacted with a dummy for the person being male. The other interaction terms (intermarriage sex ratio, where it is not omitted, and intermarriage sex ratio interacted with a male dummy) are included in the regression, but not reported. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share	0.026	0.016	-0.021	0.121***	0.309**	0.304**	0.268***	0.206***
	(0.50)	(0.30)	(-0.47)	(2.97)	(2.82)	(2.74)	(3.77)	(3.30)
Race Share * Abnormal	-0.140***	-0.144***	-0.103***	-0.021				
Intermarriage Rate	(-5.34)	(-5.42)	(-4.33)	(-0.94)				
Race Share * InterMarriage Sex					-0.180	-0.119	-0.215**	-0.135
Ratio					(-1.38)	(-0.87)	(-2.33)	(-1.50)
Race Share * InterMarriage Sex					0.235***	0.115***	0.127***	0.141***
Ratio * Male					(5.78)	(4.77)	(5.61)	(5.77)
Sex*State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics*(State, Year) FE	Y	N	N	N	Y	N	N	N
Sex*Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
Sex*Area Traits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Sex * Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	11,853,697	11,853,691	11,853,691	11,853,691	11,853,697	11,853,691	11,853,691	11,853,691
R-squared	0.408	0.425	0.427	0.429	0.409	0.425	0.427	0.430

Table 10 – Trust and Birth Rates

This Table examines how local measures of trust affect the relationship between racial isolation and the number of children a woman has. In Panel A, we consider the generalized state-level trust measures in 2006, from Putnam (2007). These are compared to state-level race share measures (in columns 1-6) and baseline local area measures (in columns 7-12). The years examined are either all years, only the years between 2001 and 2010, or 2006 only. In Panel B, we consider the county-level measures of social capital from Chetty et al. (2022), namely the volunteering rate, friendship clustering, and economic connectedness. These are compared with county-only measures of race share. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Social Capital Survey Direct Trust Measure												
Race Share (State)	0.358***	0.282***	0.353***	0.267***	0.328***	0.243***						
	(7.42)	(7.11)	(6.50)	(5.85)	(5.81)	(4.79)						
State Level Trust		0.432***		0.465***		0.449***		0.326**		0.350***		0.359***
		(4.38)		(4.77)		(4.43)		(2.77)		(2.99)		(3.03)
Race Share (Base)							0.294***	0.274***	0.286***	0.264***	0.283***	0.262***
							(9.61)	(9.84)	(7.85)	(7.65)	(8.78)	(8.62)
Years	All	All	2001-2010	2001-2010	2006	2006	All	All	2001-2010	2001-2010	2006	2006
Demographics *Year FE	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y	N
Observations	9,179,897	9,179,897	3,154,144	3,154,144	414,474	414,474	5,967,596	5,914,400	1,956,940	1,945,083	322,868	321,148
R-squared	0.384	0.384	0.369	0.370	0.371	0.372	0.393	0.394	0.381	0.382	0.377	0.378

Table 11 – Similarity In Other Variables and Number of Children

This table examines how diversity in other demographic variables is associated with different numbers of children. For each demographic variable, we take as the independent variable the fraction of residents in the local area who share the same value of the trait as the woman. Local area is taken as county, then city if county is missing, then detailed metro area if both city and county are missing. Panel A examines education, income and age. Education is a dummy variable for the highest level of schooling (e.g. high school, college, graduate degree). Income is deciles of income across the US in the year in question. Age is the fraction of the population that is between two years younger and ten years older than the woman. Panel B examines country of birth and citizenship. Country of birth is a dummy for whether the person was born in the US, and Citizenship is a dummy for whether the person is a US citizen. Panel C includes these variables together, and also computes the marginal effect of a one-standard deviation unconditional change in each of the variables for each specification. All other control variables are defined in Table 2. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Panel A - Education, Income, Age											
Education Share	0.102*	0.176**	-0.010	-0.011								
	(1.97)	(2.72)	(-0.30)	(-0.33)								
Income Decile Share					0.659***	0.789***	0.733***	0.740***				
					(7.57)	(10.06)	(10.73)	(10.94)				
Age (-2,+10) Share									-0.771***	-0.513***	0.475***	0.504***
									(-4.98)	(-5.09)	(3.33)	(3.50)
State-Year FE	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N	N	N	Y	N
Area Traits * Year	N	N	Y	N	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.391	0.399	0.404	0.405	0.392	0.400	0.404	0.405	0.392	0.399	0.404	0.405

Panel B - Country of Birth, Citizenship								
US Born Share	0.152***	0.178***	-0.024	-0.025				
	(4.20)	(4.35)	(-1.17)	(-1.21)				
Citizenship Share					0.354***	0.405***	0.047	0.046
					(10.91)	(7.73)	(1.25)	(1.21)
State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
Area Traits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.391	0.399	0.404	0.405	0.392	0.400	0.404	0.405

Panel C - Other Variables in Combination								
	Coefficients				Effect of 1 s.d. unconditional change			
Race Share	0.194***	0.226***	0.188***	0.191***	0.063	0.073	0.061	0.062
	(9.98)	(7.30)	(5.96)	(5.99)				
Education Share	0.007	0.013	-0.090***	-0.092***	0.001	0.002	-0.013	-0.013
	(0.17)	(0.29)	(-3.09)	(-3.14)				
Income Decile Share	0.609***	0.678***	0.640***	0.645***	0.022	0.024	0.023	0.023
	(8.20)	(10.78)	(10.44)	(10.73)				
Age (within 10 years) Share	-0.605***	-0.296**	0.402***	0.429***	-0.032	-0.016	0.021	0.023
	(-3.39)	(-2.84)	(2.85)	(3.01)				
US Born Share	-0.401***	-0.345***	-0.166***	-0.167***	-0.100	-0.086	-0.041	-0.042
	(-8.48)	(-5.61)	(-5.60)	(-5.34)				
Citizenship Share	0.677***	0.625***	0.175***	0.177***	0.210	0.194	0.054	0.055
	(11.35)	(10.54)	(4.13)	(3.86)				
State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
AreaTraits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585				
R-squared	0.394	0.401	0.405	0.406				

Table 12 – Time Series Effects of Diversity on Fertility

This Table examines how time series changes in the average local level of diversity (measured across the US) are associated with changes in US birth rates. The dependent variable is the average across all respondents of race share, either measured using combined geography (i.e. county, then city if county is unavailable, then detailed metro area if both city and county are unavailable), city only, county only, or state. In Panel A, the dependent variable is the total fertility rate in the year after the diversity measure, taken from the St Louis Fed FRED database. Additional controls are included for the level of inflation, unemployment, and GDP growth. The first five columns use OLS regressions with data back to 1971. The last five use Newey-West regressions with five lags, and data from 2006. "Full Sample Change" is the change in the independent variable (i.e. TFR) over the period in question. "Predicted Change" is the regression coefficient multiplied by the change in the independent variable from the first sample year to the last. "Fraction of Change Explained" is the ratio of these numbers. In Panel B, the dependent variable is the unadjusted average number of children for all women in the survey year.

Panel A - Total Fertility Rate and Economic Controls

Race Share (Base Combined)	1.440***	2.569***				3.969***	3.920***			
	(3.86)	(5.10)				(10.29)	(9.11)			
Race Share (City Only)			2.470					7.902***		
			(1.50)					(10.92)		
Race Share (County Only)				2.090**					6.808***	
				(2.73)					(12.50)	
Race Share (State)					2.570***					4.433***
					(4.69)					(8.49)
Inflation		-0.042**	-0.020	-0.024	-0.043**		0.001	0.008	0.010	0.002
		(-2.70)	(-0.80)	(-1.10)	(-2.56)		(0.03)	(0.55)	(0.70)	(0.13)
GDP Growth		-0.011	-0.011	-0.008	-0.005		0.005	0.008	0.019**	0.012
		(-0.74)	(-0.54)	(-0.44)	(-0.30)		(0.55)	(0.74)	(2.86)	(1.39)
Unemployment		0.329	0.152	0.094	0.748		0.598*	0.479	0.203	0.902**
		(0.22)	(0.07)	(0.04)	(0.48)		(1.93)	(1.11)	(0.51)	(2.90)
First Year	1971	1971	1981	1971	1971	2006	2006	2006	2006	2006
Method	OLS	OLS	OLS	OLS	OLS	NW	NW	NW	NW	NW
Full Sample Change	0.602	0.602	0.148	0.602	0.602	0.444	0.444	0.444	0.444	0.444
Predicted Change	0.393	0.702	0.364	0.513	0.704	0.426	0.421	0.462	0.512	0.424
Fraction of Change Explained	0.653	1.166	2.458	0.852	1.170	0.960	0.948	1.040	1.153	0.954
Observations	21	21	19	20	21	16	16	16	16	16
R-squared	0.439	0.643	0.159	0.381	0.605	0.886	0.892	0.865	0.924	0.880

Panel B - Number of Children, Long Sample								
Race Share (Base Combined)	0.853***				0.644**			
	(6.51)				(2.32)			
Race Share (City Only)		0.630***				0.478**		
		(6.25)				(2.62)		
Race Share (County Only)			2.021***				1.767**	
			(8.84)				(3.62)	
Race Share (State)				0.791***				0.597**
				(6.31)				(2.30)
First Year	1850	1850	1950	1850	1850	1850	1850	1850
Years Included	All	All	All	All	Decades	Decades	Decades	Decades
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Full Sample Change	0.496	0.496	0.018	0.496	0.517	0.517	0.039	0.517
Predicted Change	0.426	0.349	0.692	0.415	0.314	0.264	0.583	0.304
Fraction of Change Explained	0.858	0.704	38.916	0.836	0.607	0.511	15.053	0.588
Observations	32	30	22	32	16	15	7	16
R-squared	0.585	0.583	0.796	0.571	0.278	0.346	0.724	0.274

Data Appendix

U.S. Data

U.S. Census data sources are taken from the IPUMS default samples for each year, namely:

1% sample from 1850, 1860, 1870, 1880, 1900, 1910, 1920, 1930, 1940, 1950 and 1960

1% metro fm1 sample from 1970

1% metro sample from 1980 and 1990

1% sample from 2000

10% sample from 2010

ACS surveys from 2000-2021

International Data

Data for the number of samples, observations, control variables and racial classifications for each of the countries are listed below.

Country	# Obs.	Mean # Children	# Samples	# Course Geography Units	# Fine Geography Units	Control Variables	Races	Number	Pct
Brazil	22,877,029	1.646	6	25	2,040	Age, Education,	White	10,232,595	54.92
						Employment,	Black	1,202,607	6.46
						Marital Status,	Indigenous	40,650	0.22
						Race, Urban	Asian	126,174	0.68
							Brown	7,028,211	37.72
Colombia	2,215,042	1.542	4	22	438	Age, Education,	White	561,681	82.77
						Employment,	Black	74,113	10.92
						Marital Status,	Indigenous	41,884	6.17
						Race, Urban	Other	889	0.13
Costa Rica	231,878	1.499	4	7	55	Age, Education,	White	138,357	93
						Employment,	Black	2,232	1.5
						Marital Status,	Indigenous	1,108	0.74
						Race, Urban	Asian	156	0.1
							Chinese	154	0.1
							Mulatto	6,015	4.04
							Other	755	0.51
Cuba	383,755	0.970	2	14	137	Age, Education,	White	242,150	63.1
						Employment,	Black	35,451	9.24
						Marital Status,	Mixed Race	106,154	27.66
						Race			
Ecuador	909,119	1.601	5	14	79	Age, Education,	White	39,427	8
						Employment,	Black	7,513	1.52
						Marital Status,	Afro-Ecuadorian	12,233	2.48
						Race, Urban	Indigenous	30,938	6.28
							Mestizo	370,966	75.29
							Mulatto	11,909	2.42
							Other	1,753	0.36
							Montubio	17,969	3.65
El Salvador	201,637	1.499	2	14	103	Age, Education,	White	14,437	13.32
						Employment,	Black	117	0.11
						Marital Status,	Indigenous	261	0.24
						Race, Urban	Mestizo	92,930	85.75
							Other	623	0.57
Jamaica	121,582	1.404	3	14	N/A	Age, Education,	White	229	0.2
						Employment,	Black	103,095	88.49
						Marital Status,	Chinese	213	0.18
						Race, Urban	Indian	1,559	1.34
							Other Asian	13	0.01
							Mixed Race	11,304	9.7
							Other	98	0.08

Country	# Obs.	Mean # Children	# Samples	# Course Geography Units	# Fine Geography Units	Control Variables	Races	Number	Pct
Mozambique	651,821	1.932	2	11	143	Age, Education,	White	457	0.07
						Employment,	Black	633,924	99.35
						Marital Status,	Indian	489	0.08
						Race, Urban	Pakistani	57	0.01
							Mixed Race	3,037	0.48
							Other	135	0.02
South Africa	3,141,423	1.042	5	5	19	Age, Education,	White	155,624	6.39
						Employment,	Black african	2,014,603	82.71
						Marital Status,	Asian	53,757	2.21
						Race, Urban	Coloured	208,378	8.56
							Other	3,288	0.13
United Kingdom	92,397	1.020	1	11	N/A	Age,	White	86,347	93.45
						Employment,	Black African	499	0.54
						Marital Status,	Black Caribbean	1,084	1.17
						Race	Other Black	350	0.38
							Chinese	388	0.42
							Indian	1,673	1.81
							Pakistani	820	0.89
							Bangladeshi	197	0.21
							Other Asian	504	0.55
							Other	535	0.58
Uruguay	282,446	1.229	6	19	67	Age, Education,	White	80,648	88.89
						Employment,	Black	3,433	3.78
						Marital Status,	Indigenous	1,529	1.69
						Race, Urban	Asian	183	0.2
							Mestizo	917	1.01
							Two or More Races	3,940	4.34
							Other	82	0.09
Zimbabwe	123,039	1.400	1	10	88	Age, Education,	White	145	0.12
						Employment,	Black	122,537	99.67
						Marital Status,	Asian	84	0.07
						Race, Urban	Mixed Race	170	0.14
							Other	8	0.01